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CHEMISTRY,

AND

THE ARTS.

VOL. XXXI.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers and Communications in the present Volume are Mrs. Agnes Ibbetson; Luke Howard, Esq.; Richard Lovell Edgeworth, Esq. F. R. S. M. R. I. A.; A. Z.; Mr. George John Singer; L. O. C.; Mr. Benjamin Cook; Mr. John Murray; A Lover of the Modern Analysis; James Clarke, M. D. late Physician to the Nottingham General Hospital; George Pearson, M. D. F. R. S. &c.; Mathematicus; the Right Hon. Lord Gray; Alexander Marcet, M. D. F. R. S. one of the Physicians to Guy's Hospital; A. B. C.; J. Phœnix; Benjamin Smith Barton, M. D. Mem. of the Am. Phil. Soc. &c.; J. D. Maycock, M. D.; John Davy, Esq.; and a Correspondent.

Of Foreign Works, M. Henry; M. Klaproth; Mr. J. E. Berard; M. Gay-Lussac; M. Guyton-Morveau; M. Theodore de Saussure; M. Bucholz; M. Hildebrandt; M. Reuss; M. Vauquelin; M. Huberle; M. Marcel de Serres; and Dr. Francis Delaroche.

And of British Memoirs abridged or extracted, Mr. J. Allan; William Thomas Brande, Esq. F. R. S.; James Parkinson, Esq.; Mem. of the Geol. Soc.; Mr. William Salisbury; William Hyde Wollaston, M. D. Sec. R. S; Alexander Marcet, M. D F. R. S. &c.; Mr. Adam Reid; Mr. George Spark; Thomas Andrew Knight, Esq. F. R. S. &c.; Mr. John Maher, F. H. S.; A. Hawkins, Esq.; William Fitton, M. D.; William Charles Wells, M. D. F R. S.; Mr. Benjamin Cook; Mr. H. B. Way; Smithson Tennant, Esq. F. R. S. &c.; and W. H. Pepys, Esq. F. R. S. &c.

The Engravings consist of 1. Delineations from Nature illustrative of the Mechanism of Leaves, by Mrs. Agnes Ibbetson. 2. Mr. Allan's Mathematical Dividing Engine. 3 and 4. Delineations from Nature of different Parts of Flowers, to illustrate their Mechanism, by Mrs. Agnes Ibbetson. 5. Figures to illustrate some Diseases of Vegetables, delineated from Nature, by Mrs. Agnes Ibbetson. 6. Appearances from Sugar in the Serum of Blood, by W. H. Wollaston, M. D. Sec. R. S. 7. A Compensation Pendulum, by Mr. Adam Reid. 8. Mr. G. Spark's Noctuary, or Apparatus for indicating the Hour in the Dark by Means of a common Watch. 9 and 10. Dissections of Aquatic and Semiaquatic Plants, delineated from Nature by Mrs. A. Ibbetson. 11. Apparatus for making Gas and various other Products from Pitcoal, by Mr. Ben. Cook. 12. Method of procuring Turpentine from British Firs, by Mr. H. B. Way. 13. Apparatus for exposing Animals to a heated humid Atmosphere, by Dr. Francis Delaroche.

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JANUARY, 1812.

ARTICLE I.

On the Mechanism of Leaves. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

TO prove so important a point, as that all plants are governed by mechanical means, it will not I hope be thought superfluous, to give a specimen of the sort of mechanism belonging to each part of a plant. I have already shown in what manner the mechanical power increases progressively from the firs, which have no spiral wire, to the sensitive plant, which has such a complicated arrangement. I have also shown the mechanical use of the gatherers of the leaves, whether one or two. I shall in the present letter therefore point out the means, by which leaves embracing the stem have also the power of turning and changing their position, equalling in this respect those which I described in my last letter: I shall next point out those stems which are formed like joints, and which may be said wholly to differ in mechanism from the former, turning on a ball and socket: these are most of the galia, arenaria, stel- Subject of the Letter.

 laria,

lariæ, and also many of the twining plants: I shall then show the manner in which most leaves that are nearly sessile are conducted, and what sort of motion they possess, giving as an example the ericæ; and shall finish by the evidence of a few more leaves, to prove, that there are hardly any without some sort of mechanism.

Mechanical arrangement of leaves embracing the stem.

To begin with the species of leaves, which embrace the stem, I shall produce as an example one of the umbelliferous tribe; the smyrnium olusatrum. This plant has at the end of the leaf a large hood, which I shall call the protector; because it not only contracts and dilates like a gatherer (the spiral wire running in stripes through it) but serves as a guard to the buds; which, forming in bunches, stay not within the bark, as the buds of trees, till fit to issue forth, but shoot, like all annuals and herbaceous plants, directly from the line of life to the exterior of the rind. Nature seems therefore to have placed this sort of cover (see Pl. 1, fig. 1, *a a*) which grows with its growth, and clings close to it, as a succedaneum for the stem, which in trees covers and conceals the bud for a long time. And it can scarcely be conceived what a perfect protection it is from the frosts of spring, as, like all leaves, it has on each side that impervious skin or cuticle, which no rain or cold winds can pierce, and no moisture pervades, but that which passes through the hairs into the leaf, and that which evaporation gives it. Most of the pentandria digynia tribe are formed *thus*, at least with leaves possessing this species of mechanism. It may easily be seen how much the spiral wire is contracted and turns, since, whether *open* or *shut*, the protector is continually twisted in a double manner round the stem; nor does the large bunch of flowers leave its close drawn curtain, till just before the corollas open. Being umbelliferous, large branches of flowers shoot at once; as soon as the cover is withdrawn, new life as well as light seems to be given to the plant within, which is, when first opened, generally found covered with a white powder, probably the result of evaporation. Of this I shall hereafter give a farther account. But when the leaflets in this plant increase to a great length (which they will often do to ten or twelve leaves of a side) though they in some measure resemble

Pentandria digynia plants.

resemble the ash, they are governed by a very different species of mechanism: they have a swelling down each side of the stalk, in which the spiral wire runs in a groove (see fig. 2, *b b*) which corresponds with the sort of knob round which the wire of each leaf is turned; communicating all the way with a knot and pulley till it reaches the protector at *c c*, fig. 1.

The management of the galia, and most of those plants, the stalks of which enlarge where the leaves meet, is contrived in a curious manner, with a sort of mechanism consisting of a ball and socket. No part of a plant (the seed excepted) has given me so much trouble as this; having found two sorts of balls in vegetable life, and confounded the two together, viz. the ball and excrescence in trees, and the ball continually found appertaining to the mechanism of plants. I have at last learned to distinguish them. Of those in trees I shall soon give a description, when entering again on the subject of the wood in plants; which will develope many hidden secrets, that ought to be explained, and illustrate many things that may have appeared contradictory. Respecting the ball, which forms a part of the mechanism in plants, each stem has one, on which it turns like the knee of a quadruped: but, to see the ball, the plant must be taken at a very early age, for it is the first part that decays, and in the phaseolus vulgaris I have known it gone before the flower disappeared; and I have often seen five or six stems turn in one collected set of joints. When old they stiffen, and a sort of matter fills up the instertices of the sockets. They then become immovable; but before this, if you move the stem, you may see them turn, but cannot turn them yourself without breaking the spiral wire. See fig. 3, and a section at fig. 4. This specimen will at least discover the manner in which the spiral wire is conveyed from one ball to another, communicating its influence, and spreading its power from stem to stem. Mechanism of the ball and socket.

I must now correct a mistake I before made in saying, that there was no spiral wire in the ericæ. It is so diminutive, and lies so low in the groove, that I overlooked it. The leaves of the heaths, though very sessile, still possess a motion to and from the plant. See fig. 5, *c c*. They are The ericæ possess the spiral wire.

 placed

placed so much in the manner of the fir leaves, that I was deceived by it. But in the firs the rind and bark are all leaves only, whereas in the ericæ the leaves are wholly detached, nor are they quite sessile, as may be seen at fig. 5, *d d*. I shall now show the manner in which almost all leaves that are evergreen, and have a shining surface, are formed in their peduncle, having but one gatherer, which embraces the whole length of the leaf stalk. The upper part alone puckers, as the motion requires only this part to be gathered up, when the leaf is to be raised close to the stem; or stretched out, when it is to fall back: See fig. 7, where *c* is the back and *f* is the front.

Beauty of the leaf bud when within the stem.

I have already said, that almost all leaves of trees have one or two gatherers; if only one, it is *that adjoining the stem*, as it is *that* in which the buds of both sorts are contained; nothing is more easy than to know the leaf from the flower bud, even before they leave the stalk; the leaf bud is so peculiar in its appearance, it is impossible to mistake it; it is composed of a quantity of hairs, or vessels, which are already beginning to weave the leaf. It is more than four times the size of the flower bud, and in trees generally of a bright brown colour, very shining and beautiful. See fig. 6, *g g*. I hardly know a subject more worth studying in the solar microscope *than this*, if taken progressively from its first beginning to its leaving the stalk as a bud, for it is in this early state it shows the whole process of weaving the leaf, as it protrudes not from the stalk till the form is complete. Though Linneus admired much the manner of pressing and folding the leaf, he was totally ignorant of its beautiful commencement. I have given the lower gatherer of the tilia, fig. 6. It is very curious to see, when the flower buds are many in number, (fig. 6, *h h*,) how the wood vessels and spiral wire will meander round them, that they may not injure by passing over them; for if, while resting on the buds, they were suddenly *contracted*, they would probably divide, and thus destroy them. I think I have now shown the mechanism of most leaves, my next letter will give the mechanical management of flowers.

I am, Sir, your obliged servant,

AGNES IBBETSON.

II.

II.

Improvements in a Mathematical Dividing Engine: by Mr. J. ALLAN, of Blewit's Buildings, Fetter Lane.*

SIR,

I beg leave to send to you, herewith, for the inspection of the Society of Arts &c., a model of my improvement on the mathematical dividing engine, which I have lately made, containing that part which differs in principle from those made by the late Mr. Ramsden and others; the drawings or engravings of which are, I suppose, in the Society's possession. I therefore am of opinion the Society will think, that the wooden wheel I have sent, with the movable ring on its edge, will be sufficient to demonstrate its good effect in correcting the teeth or rack where the screw acts. You will please to observe, that it is cut by a screw cutter, and it is required to go many times round the engine before the teeth are full. To effect this, I reversed the movable ring not less than twenty times, so that I have not the least doubt of the one ring having corrected the other to a degree of perfection, which had not hitherto been obtained in engines.

Improvement in a mathematical dividing engine.

This simple, easy, and correct way of making engines, may be applied with great advantage to circular instruments, for the purposes of astronomy and land surveying. If the Society will do me the honour to appoint a Committee to view the engine itself, I will demonstrate its effects.

I am, Sir,
Your very humble servant,
JAMES ALLAN,
Divider of Mathematical Instruments.

No. 12, *Blewit's Buildings, Fetter Lane,*
Nov. 20, 1809.

Mr. Allan's Description of his Mathematical Dividing Engine, and his Method of forming it.

My engine is of belmetal, thirty inches in diameter. I turned a brass ring about three sixteenths of an inch thick,

The engine described.

* Trans. of the Soc. of Arts &c., vol XXVIII, p. 179. The gold medal was voted to Mr. Allan for this improvement.

and

and fitted in on the underside of the above belmetal wheel, which I made fast by twenty four rivets; I then fixed in the axis, and turned the wheel and ring together on the lathe, as near as possible to the required shape on its own axis. This being done, and having mounted it on its own stand, where it now acts, I fixed a tool, with an adjustment to turn the edge of the belmetal wheel where the uppermost or movable ring of the same thickness as the other is fitted on; for if the circle, where the movable ring fits the belmetal, is not turned as true as possible, (which cannot be done properly by any other means than by a fixed tool) the movable ring will not reverse correctly. When this was done, I fitted on the movable ring. I then divided the lower under ring into twenty four parts, for the screws which keep the rings together. I also divided it into four parts for the steady pins; the holes of which I made by an upright drill fixed and adjusted for the purpose. I then cut two opposite divisions, in order to reverse the uppermost ring correctly, which were my guide in broaching for my steady pins, and which I did with a broach to a stop fixed on it. In broaching I reversed the movable ring many times, taking care at the same time that my opposite divisions were correct.

My first idea was to have two wheels or circles, acting on the same centre, so as to constitute a double edge, to afford me an opportunity to reverse in the act of cutting the rack or teeth; but I thought the method in which I have done it would with care be equally correct. Either of the methods come to the same point, and I preferred the way I have employed, thinking it the least expensive. By this self-correcting method, instruments may be made for astronomical purposes, racked and divided on their own centre, and if carefully done would border on perfection itself, consequently I consider it to be the greatest improvement ever made in the art of dividing. I call it self-correcting, because every time it is reversed in cutting the teeth, the screw has a fresh opportunity to correct errours insensible to the eye.

Small circles of great accuracy.

I have well considered the subject, and think, that a circle of twelve inches diameter, made on this principle, would measure angles equally, if not more accurately, than astronomical

nomical instruments divided by engines, or by any other methods hitherto used by instruments of any size. It is, therefore, my opinion, that the supposed necessity of making very large circles, for the sake of obtaining correct divisions, will be done away.

JAMES ALLAN.

CERTIFICATES.

After a close consideration of Mr. Allan's improvement in dividing engines, (I mean his mode of racking the teeth only) when combined with the methods at this time known and practised, I look upon it as an important discovery; it is a plan, that in my opinion will admit of a great degree of accuracy, approaching nearly to perfection itself, particularly in circles of small radius, but not quite so applicable in large machines for the purpose of dividing. Certificates of the utility of the improvement.

JOHN STANCLIFFE.

Little Mary-le-Bone Street, Dec. 15, 1809.

SIR,

The method you have taken to produce a perfect equal racking, for the constructing an accurate dividing engine, is the greatest advance toward perfection that has been communicated to the public within my knowledge; and I believe it to be a method never before practised in this country. It is applicable to the construction of machines of any dimensions, that mathematical or nautical instruments can be graduated by.

It is my belief, that the greater number of the machines now in use are far short of the perfection they are reputed to have. Machines now in use less perfect than supposed.

I am, Sir,

Your humble servant,

M. BERGE.

Piccadilly, Jan. 8, 1810.

Mr. J. ALLAN.

Reference to the Drawing of Mr. Allan's improvement on the Dividing Engine of Ramsden, Pl. II.

The dividing engine invented by Mr. Jesse Ramsden, and for which he received the reward of the Board of Longitude, in the year 1775, is minutely explained in a quarto pamphlet, Nature of the improvement.

pamphlet, published by order of the Commissioners of Longitude; also, in the article *engine*, in Dr. Rees's New Cyclopædia, as well as some other works of a similar nature; it therefore becomes unnecessary for the Society to give any more of Mr. Allan's engine in their drawings than is explanatory of the improvement, the engine being used in the same manner as Ramsden's; this part is the great circle, upon which the arch to be divided is placed, and the circle turned about a determinate quantity at each division, by means of a screw, the threads of which engage fine teeth, cut around the periphery of the circle. The improvement by Mr. Allan consists in the method of cutting or racking these teeth, to ensure their being perfectly of equal size, in all parts of the circle.

Description of the plate.

The plan, fig. 1, inp late II, represents the upper surface of a belmetal circle mounted upon an axis, A, fig. 2, and its surface made truly plane, and perpendicular to the axis; the section shows the figure of the axis, and the central ring B, to give the greatest strength to the circle; C is a section of a portion of the frame of the engine; and D a socket into which the axis A is fitted; the circumference of the large circle is turned to such a figure as to receive a ring of brass, *a*, fig. 3, which is united firmly to it by a number of pins, one of which is shown in the figure. Upon this ring a second, *b*, is placed, the two making the same thickness as the circle. The inside of the ring *b*, and the outside of the belmetal circle, are fitted to each other with the utmost accuracy, and great care taken to turn the said fitting truly concentric with the axis of the circle; the brass rings *a* and *b* are held together by twenty four screws, as shown in the plan; and a groove, corresponding to the curvature of the screw which moves the circle, is turned in the outside of the two; in this state the racking of the teeth is performed by a screw similar to that afterward used to turn the circle to its divisions, but notched across the threads, so that it cuts like a saw, when pressed against the circle and turned round, and removes the metal from the spaces between the teeth, which are by this means formed around the edge of the circle; when this has been performed all round, two fine lines are drawn across the brass and belmetal circles, diametrically opposite

opposite each other; the twenty four screws are then withdrawn, and the upper brass ring turned exactly half round, which is determined by the lines before mentioned; and by this means the teeth of the circle are divided into two thicknesses, and being put together again in opposite directions, if any error arose in racking the teeth, it would be shown by the upper and lower halves of the teeth not coinciding when reversed, and by racking them while reversed the screw would cut away the inequalities, and make all the teeth of the same size and distance from each other; this reversing the teeth is performed several times, till the teeth are brought to a perfect equality in all parts of the circle; four steady pins are accurately fitted into the two rings to hold them together in any of the positions in which they have been racked together, and it is upon these the dependence is placed for the coincidence of the teeth, the twenty four screws being merely to hold them fast together, and fitted rather loosely in their holes, that they may not strain the steady pins.

III.

Observations on the Waste, that Pulverization occasions in Substances: by Mr. HENRY, *Chief of the central Pharmacy of civil Hospitals, &c**.

THE School of Pharmacy being consulted by count de Cessac, *ministre directeur de l'administration de la guerre*, respecting the waste, that certain mineral, vegetable, and animal substances, employed as medicines, experience on being powdered, appointed two of its members, to examine particularly into a subject so interesting in pharmacy, and to make a report on it. Inquiry into the waste of drugs by the French Government.

Report of the Committee to the Members of the School of Pharmacy.

GENTLEMEN,

You have appointed two of your members, to make a re-

* Ann. de Chim. vol. LXXV, p. 324.

port

Hartz. He was represented as an old man, with a long beard and hideous countenance, standing barefoot on a fish rough with scales and spines, holding a wheel in his left hand, and a basket filled with flowers and fruit in his right.

On this altar the first-born of mothers was sacrificed to him, as to Moloch.

The abolition of the pagan worship by Charlemagne overturned this idol, but his altar was reserved for the use of the Christian church.

Probably fabulous. The critical history of Germany does not acknowledge any god Krodo, but takes the whole for a fable invented by the monks of the middle age.

However this may be, the altar appears to have been used for burning animals to some deity.

The altar described. The shape of the altar is a hollow parallelopipedon, three feet three inches long, two feet and a half wide, and two feet seven inches high. It stands on four feet, supported by little men of hideous aspect. It is covered with a slab of white marble.

The metal. The metal of this altar is of a brass yellow colour, a hackly fracture, and easily polished. Its specific gravity is 8·767.

Analysis of it. On 200 grains of it nitric acid was poured, which dissolved it completely, without the assistance of heat.

The solution was divided into two parts. Into one of these was dropped a solution of sulphate of soda. The precipitate, when well washed and calcined, presented 18·25 grs of sulphate of lead, answering to 13 grs of metallic lead.

The supernatant liquid was mixed with 200 grs of sulphuric acid, and evaporated to dryness. The mass was redissolved, and iron added, which precipitated 69 grains of copper.

The other half of the solution was mixed with five parts of distilled vinegar, and the mixture poured on a thin plate of hammered lead in a shallow dish.

After a few days, and by the assistance of heat, the copper was precipitated. The liquor being filtered off, the lead

lead was thrown down by sulphate of soda. The supernatant liquid was then precipitated by carbonate of soda: and the carbonate of zinc, well washed and calcined, left 22·25 grs of oxide of zinc, answering to 18 grs of metallic zinc.

The alloy of the altar therefore consists of

Copper	69
Zinc	18
Lead	13
	100.

Analysis of the alloy of the imperial seat.

In the church of Goslar there is an arm chair, called the *kaiserstuhl*. The kaiserstuhl.

The colour of the metal is a pale copper red: its fracture is porous: its spec. grav. is 8·087.

200 grains of the alloy were treated with nitric acid. 12·25 grs of oxide of tin were left. From the decanted liquor sulphate of soda threw down 7·5 grs of sulphate of lead. From the remaining liquid, after the addition of sulphuric acid in excess, iron separated 185 grs of copper. Analysis of it.

This therefore is composed of

Copper	92·5
Tin	5
Lead	2·5
	100.

Analysis of a large chandelier.

200 grs were treated by nitric acid, and the solution divided into two parts. Analysis of an antique chandelier.

In one of these sulphate of soda occasioned no precipitate: but iron separated from it 84 grs of copper.

The other half was treated as in the first analysis, and as much oxide of zinc was obtained, as answered to 16 grs of the metal.

Consequently

Consequently this alloy consists of

Copper	84
Zinc	16
	100.

IV.

An Account of a Vegetable Wax from Brazil. By WILLIAM THOMAS BRANDE, *Esq., F. R. S.**

Vegetable wax from Brazil.

SECT. I. THE vegetable wax, described in this paper, was given to the president by Lord Grenville, with a wish, on the part of his Lordship, that its properties should be investigated, in the hope that it might prove a useful substitute for bees wax, and constitute, in due time, a new article of commerce between the Brazils and this country.

Tree whence it is obtained.

It was transmitted to lord Grenville from Rio de Janeiro, by the comte de Galveas, as a new article lately brought to that city, from the northernmost parts of the Brazilian dominions, the capiteneas of Rio Grande and Seara, between the lattitudes of three and seven degrees north: it is said to be the production of a tree of slow growth, called by the natives *carnauba*, which also produces a gum used as food for men, and another substance employed for fattening poultry.

When the comte wrote to Lord Grenville in July last, orders had been sent to the governors of the districts where it grows, requiring them to report more particularly on the nature and qualities of this interesting tree; we may therefore hope, that information will soon be obtained, whether the article can be procured in abundance, and at a reasonable price, in which case it will become a valuable addition to the comforts of mankind, by reducing the price and improving the quality of candles, flambeaux, &c.

Not the ceroxylon andi-

The article, in the state in which it was sent, resembles much that described by Humboldt, as the produce of the

* Phil. Trans. for 1811, p. 261.

the ceroxylon andicola*; but it is not likely to be the same, as Humboldt's wax is collected from a stately palm tree, which grows on the high mountains, from 900 to 1450 toises above the level of the sea, and on the edge of the regions of perpetual snow. On the other hand, the Brazilian plant is described as a slow growing tree, but not as a large one, and there are no hign mountains delineated in the most accurate and recent maps of the capitenеas where it is found. But a more decisive argument against their identity is the analysis of Vauquelin, published by Humboldt, which shows, that the produce of the ceroxylon consists of two thirds resin and only one third wax; but the Brazilian article is entirely wax, and affords not the smallest trace of resin. The Brazilian plant, however, was not entirely unknown to Humboldt, for it appears from his book, that Mr. Correa had informed him, that a palm, called *carnauba* by the natives of Brazil, produced wax from its leaves. cola of Humboldt.

SECT. II. 1. The wax, in its rough state, is in the form of a coarse pale gray powder, soft to the touch, and mixed with various impurities, consisting chiefly of fibres of the bark of the tree, which, when separated by a sieve, amount to about 40 per cent. The wax described.

It has an agreeable odour, somewhat resembling new hay, but scarcely any taste.

At 206° Fahrenheit it enters into perfect fusion, and in this state it may be farther purified, by passing it through fine linen. By this process, it acquires a dirty green colour, and its peculiar smell becomes more evident. When cold, it is moderately hard and brittle. Its specific gravity is 0·980.

2. Water exerts no action on the wax, unless boiled with it for some hours; it then acquires a slight brown tinge, and the peculiar odour of the wax. Insoluble in water,

3. Alcohol does not dissolve any portion of the wax, unless heat be applied. and in alcohol unless heated.

Two fluid ounces of boiling alcohol, spec. grav. 0·826, dissolve about ten grains of the wax, of which eight grains are deposited as the solution cools, and the remaining two

* Plantes equinoctiales, p. 3.

grains

grains may be afterward precipitated by the addition of water, or may be obtained unaltered by evaporating the alcohol.

The solution of the wax in alcohol has a slightly green tinge.

Little soluble in ether.

4. Sulphuric ether, spec. grav. 0·7563, dissolves a very minute portion of the wax, at the temperature of 60°.

Two fluid ounces of boiling sulphuric ether dissolve thirty grains of the wax, of which twenty-six grains are deposited by cooling the solution, and the remaining four grains may be obtained by allowing the ether to evaporate spontaneously.

Very soluble in oil.

5. The fixed oils very readily dissolve the wax at the temperature of boiling water, and form with it compounds of an intermediate consistence, very analogous to those which are obtained with common bees wax.

The compound soluble in ether, and slightly in alcohol.

In examining some combinations which I had made of the vegetable wax with olive oil, I was surprised to find them perfectly soluble in ether, and sparingly soluble in boiling alcohol.

As it is commonly stated, that the fixed oils are insoluble in ether and in alcohol, I was led to attribute the solution of the oil, in these instances, to its being combined with the wax; but subsequent experiments, of which I shall state the general results, have shown me, that these opinions are erroneous.

Fixed oils soluble in ether,

Four fluid ounces of sulphuric ether, spec. grav. 0·7563, dissolve a fluid ounce and a quarter of the expressed oil of almonds; of olive oil, the same quantity of the ether dissolves a fluid ounce and a half; of linseed oil, two fluid ounces and a half; and castor oil is soluble in any proportion in sulphuric ether of the above specific gravity.

and sparingly in alcohol.

The expressed oils of almonds and of olives are very sparingly soluble in alcohol, spec. grav. 0·820.

Linseed oil is more soluble than the two former. Four fluid ounces of alcohol, spec. grav. 0·820, dissolve nearly one fluid drachm.

Castor oil completely soluble in alcohol.

Castor oil is perfectly soluble in every proportion in alcohol, spec. grav. 0·820. In alcohol of a higher specific gravity, as 0·840, it is very sparingly soluble*.

Adulteration of essential oils.

* The solubility of castor oil in alcohol was mentioned to me some months ago by Dr. Wollaston, who also informed me, that it had on this account been employed to adulterate certain essential oils of high value, especially the oil of cloves.

As

As some of the difficultly soluble resins are more easily dissolved in alcohol, to which a small proportion of camphor has been added, I endeavoured to ascertain, whether the fixed oils were rendered more soluble by the same means, but found, that this was not the case, excepting with regard to castor oil, which, although very sparingly dissolved by alcohol of a spec. grav. above 0·840°, becomes abundantly soluble, by the addition of one part of camphor, to eight parts of the alcohol. Effect of camphor.

Boiling alcohol, spec. grav. 0·840, takes up a considerable portion of castor oil and of linseed oil; it also dissolves a small quantity of the oils of almonds and of olives; but they are copiously deposited during the cooling of the alcohol, and only a small portion retained in permanent solution. Heat increases the solvent power of alcohol.

When water is added to any of these solutions of the fixed oils in ether, and in alcohol, a milky mixture is formed, and the oil gradually separates upon the surface, without having undergone any apparent alteration. Water precipitates solutions of the oils.

6. One hundred grains of the wax were boiled for half an hour in a solution of caustic potash, spec. grav. 1·090. The solution acquired a pale rose colour, but appeared to exert no farther action on the wax, which, after having been washed with warm water, retained its fusibility and other properties. No combination therefore, similar to a soap, was produced; nor was any precipitate occasioned by the addition of acids to the rose coloured alkaline solution. Action of potash on the wax:

7. The effects produced by boiling the wax in solutions of pure soda, and of the subcarbonates of soda and of potash, were analogous to those of the caustic potash. of soda and mild alkalis:

8. Solutions of pure and of carbonated ammonia exert scarcely any action on the wax. of ammonia:

9. When the wax is boiled in nitric acid, spec. grav. 1·45. there is some escape of nitrous gas, and the colour of the wax is gradually changed to a deep yellow. of nitric acid.

When the wax is removed from the acid, and washed with hot water, it is found to have become more brittle and hard, but it still retains much of its peculiar odour.

In this state it remains insoluble in the alkalis, but they now change its colour to a very bright brown, which is de-

 stroyed

stroyed by washing with dilute muriatic acid, and its original yellow colour restored.

Neither the fusibility, nor the inflammability of the wax, is impaired by this process.

Nitric acid, diluted with eight parts of water, produces the same change in the colour of the wax as the concentrated acid.

Attempts to bleach the wax.

Having been unsuccessful in my attempts to bleach the wax in its original state, I made some experiments to ascertain whether its colour could be more easily destroyed, after it had been acted upon by nitric acid; and found, that, by exposing it spread upon glass to the action of light, it became in the course of three weeks of a pale straw colour, and on the surface nearly white. The same change was produced, by steeping the wax, in thin plates, in an aqueous solution of oximuriatic gas, but I have not hitherto succeeded in rendering it perfectly white.

Action of muriatic acid:

10. Muriatic acid has little action on the wax: when boiled upon it for some hours, it destroys much of its colour.

of sulphuric:

11. Sulphuric acid changes the colour of the wax to a pale brown, and when water is added, it becomes of a deep rose colour; the inflammability and the fusibility of the wax are slightly impaired by this process.

When heat is applied, the wax is decomposed with the usual phenomena, sulphurous acid is developed, and charcoal deposited.

of acetic:

12. Acetic acid has very little action on the wax, when cold.

When the wax is boiled in this acid, a minute portion is dissolved, and again deposited as the solution cools. By long continued boiling in acetic acid, the wax is rendered nearly white; but when it is afterward washed with water, and fused, it resumes its former colour.

and of oximuriatic gas.

13. When the wax is fused in oximuriatic gas, it is rapidly decomposed, and, parting with hidrogen and oxigen, muriatic acid and water are formed, and charcoal is deposited.

Products of distillation.

14. The results of the destructive distillation of the vegetable wax are very analogous to those of bees wax.

An acid liquor, mixed with a volatile oil, are the first products; these are succeeded by a large proportion of a butyraceous

raceous oil; and a very small quantity of charcoal, affording traces of lime, remains in the retort. During the process, a little carburetted hidrogen gas is given off.

I have not considered it necessary to dwell upon the relative proportions of these different products, as they will necessarily vary according to the rapidity, with which the distillation is conducted.

This wax differs from other varieties.

SECT. III. From the preceding detail of experiments, it appears, that, although the South American vegetable wax possesses the characteristic properties of bees wax, it differs from that substance in many of its chemical habitudes; it also differs from the other varieties of wax, namely, the wax of the myrica cerifera *, of lac †, and of white lac ‡.

Bleaching.

The attempts, which I have made to bleach the wax, have been conducted on a small scale; but from the experiments related, it appears, that, after the colour has been changed by the action of very dilute nitric acid, it may be rendered nearly white by the usual means. I have not had sufficient time to acertain, whether the wax can be more effectually bleached by long continued exposure, nor have I had an opportunity of submitting it to the processes employed by the bleachers of bees wax.

Perhaps the most important part of the present inquiry is that, which relates to the combustion of the vegetable wax, in the form of candles.

Its use in candles.

The trials which have been made, to ascertain its fitness for this purpose, are extremely satisfactory; and, when the wick is properly proportioned to the size of the candle, the combustion is as perfect and uniform, as that of common bees wax.

The addition of from one eighth to one tenth part of tallow is sufficient, to obviate the brittleness of the wax in its

* Vide Dr. Bostock's Experiments on the Wax of the Myrica cerifera, in Nicholson's Journal for March, 1803; vol. IV, p. 130.

† Vide Analytical Experiments and Observations on Lac, by Charles Hatchett, Esq. F. R. S., in the Philosophical Transactions for 1804; or Journal, vol. X, p. 46, 95.

‡ Vide Observations and Experiments on a Waxlike Substance from Madras, by George Pearson, M. D. F. R. S., in the Philosophical Transactions for 1794.

 pure

pure state, without giving it any unpleasant smell, or materially impairing the brilliancy of its flame. A mixture of three parts of the vegetable wax, with one part of bees wax, also makes very excellent candles.

VI.

Observations on the Alkaline Oxalates and Superoxalates, and particularly on the Proportions of their Elements: by Mr. J. E. BERARD*.

Dr. Thomson on oxalic acid and the oxalates.

DR. Thomson has just published in the Philosophical Transactions† a very interesting paper on oxalic acid. One part of it is dedicated to the determination of the proportions of the oxalates, and for this he has taken the following method.

After having carefully ascertained the proportions in oxalate of lime, by combining a known quantity of lime with oxalic acid, he could find the quantity of real acid contained in a given weight of a solution of oxalic acid.

He then took 100 grains of a solution of oxalic acid, containing 7 grains of real acid; and, neutralizing it successively by the different alkalis, he ascertained the quantity of oxalate produced.

He did not examine the superoxalates,

But it is well known, that this acid has the property of forming with some bases salts with excess of acid: and, as the method of Dr. Thomson could not make known their proportions, he did not examine these. In the next place, I observed in his table some proportions, which could not agree with the capacities of the alkalis for saturation hitherto observed.

and some of his proportions doubted.

The subject therefore revised.

These considerations induced me, to repeat the analyses of the oxalates, and to examine particularly the superoxalates. I also took a method different from Dr. Thomson's, when it was necessary and practicable: because, if I obtained the same results in another mode, this would increase our confidence in them.

* Ann. de Chim. vol. LXXIII, p. 263. Read to the Institute, Jan. the 29th, 1810.

† Phil. Trans. 1808, p. 63: or Journ. vol. XXI, p. 14, 86.

Oxalate

Oxalate of Lime.

The proportions of oxalate of lime being to serve as a basis for all my analyses, I neglected nothing, to ascertain them with precision. Oxalate of lime examined.

As we can employ only a gentle heat to dry this salt, we can never be sure of having it entirely free from water. This is a slight source of uncertainty, which the means of chemistry have not yet enabled us to remove.

Having obtained this salt very pure, by precipitating muriate of lime with oxalate of ammonia, I dried it at the heat of boiling water, till I could no longer discover any diminution of its weight. This oxalate of lime I considered as dry. Ten gram. of this, exposed to a violent heat, left 3·8 of lime, that caused no effervescence with acids. This was the mean of four experiments. Accordingly I fixed the proportions of oxalate of lime at

62 oxalic acid,
38 lime.
———
100.

Its component parts.

The proportions found by Dr. Thompson* are very near these.

Oxalate of lime is almost wholly insoluble in an excess of its acid; whence we may infer, that there is no superoxalate of lime. No superoxalate of lime.

The following experiment seems to prove, that the oxalate of lime, as analysed by me, may be considered as very nearly free from water, without being liable to any great error.

In a glass retort I distilled some crystals of very pure oxalic acid. All the phenomena already described by Bergman† presented themselves. The crystals liquefied, and some oxalic acid was carried over into the receiver by the water of crystallization; but the matter in the retort soon became solid. A small quantity of oxicarburetted hidrogen gas was then evolved; and abundance of white vapours arose, tolerably dense, and very acrid. At the same time Oxalic acid sublimed.

* 62·5 acid, 37·5 base.

† Opuscula.

time the top of the retort became lined with fine acicular crystals of oxalic acid.

In this state the acid is very light, very white, and a little attractive of the moisture of the atmosphere. If put into water, it becomes pasty before it dissolves.

Oxalate of lime compounded of this.

From my experiments it appears, that this sublimed acid is as dry as that which exists in the oxalate of lime. I weighed 3·42 gram., dissolved them in water, neutralized them with ammonia, precipitated with muriate of lime, and carefully washed the precipitate. To collect and weigh it, I threw it on a filter, which was placed on another filter of the same weight. I then dried them gently, till I could easily separate the precipitate, which I afterward exposed to the heat of a water bath. It weighed 5·374 gr.: but the weight of the inner filter exceeded that of the outer by 0·086 of a gr.; fo that I had 5·46 gr. of oxalate of lime, which, according to the analysis I have given above, contained 3·385 gr. of acid, being nearly equal to the quantity of sublimed acid I employed*.

A second experiment confirmed these results.

Its insolubility examined.

Before I undertook the subsequent experiments, I satisfied myself, that the oxalate of lime was sufficiently insoluble, to indicate with precision the quantity of acid contained in a compound. 5 gr. [77 grs] of lime-water neutralized by muriatic acid were diluted with 400 gr. of distilled water; and in one twentieth of this oxalate of ammonia produced a precipitate, that could be perceived without hesitation. This was more than sufficient for analyses of such a nature.

But the least excess of acid dissolves a large quantity of this salt; hence I have always taken care to employ it with neutral compounds.

Crystallized oxalid acid.

Quantity of real acid in crystals of oxalic acid.

To repeat the experiments of Dr. Thomson, crystallized oxalic acid always appeared to me more commodious than a solution of this acid: and in order to ascertain the quantity of real acid contained in what I was to employ, I neutralized

* According to the proportions assigned by Dr. Thompson, it coned 3·412, which comes very near indeed to the quantity used. C.

10

10 gr. with ammonia, and precipitated by muriate of lime. Thus I obtained 11·73 gr. of oxalate of lime; so that the acid analysed contained

72·7 of real acid,
27·3 water.

100.

As this was to be employed in all my experiments, in order to have it in a uniform state it was powdered, and kept in a well stopped phial.

Oxalate of potash.

This salt is so soluble in water, that it is very difficult to crystallize it. Oxalate of potash.

10 gr. were urged in the fire in a crucible, and 6 gr. of fused carbonate of potash, insoluble in alcohol, were obtained. Now I have observed, that all the carbonates possessing these two properties are uniform in their proportions*: and I have settled the proportion of potash to carbonic acid in this salt to be as 100 to 42·42: the subcarbonate resulting from this experiment therefore contained 4·212 gr. of potash. Decomposed by fire, and

10 gr. of the same oxalate, precipitated by muriate of lime, yielded 6·543 of oxalate of lime. by muriate of lime.

The elements of the salt I analysed, therefore, were

42·12 potash,
40·57 oxalic acid,
17·31 water.

100.

Its component parts.

In another experiment, 10 gr. of the oxalic acid, analysed above, were accurately neutralized with caustic potash; the oxalate was evaporated to dryness, and exposed to a strong heat in a platina crucible; and 10·96 gr. of fused subcarbonate of potash were the result. Another experiment.

Taking a mean between these two results, I fix the proportions of dry oxalate of potash at

50·68 potash,
49·32 oxalic acid.

100.

Proportions of the dry oxalate.

* Ann. de Chim. vol. LXXI, p. 50.

Hence

Hence it follows, that 100 of potash combine with 97·3 of oxalic acid.

Superoxalate of potash.

Superoxalate of potash, or salt of sorrel.

This salt I obtained from a solution of the neutral salt, to which I had added an excess of acid. This superoxalate is known in the shops by the name of salt of sorrel. It is less soluble than the neutral oxalate.

10 gr. urged in the fire yielded 4·91 of fused carbonate of potash = 3·46 of potash.

10 gr. of the same salt, neutralized by ammonia, and precipitated by muriate of lime, yielded 10·6 of oxalate of lime = 6·58 of oxalic acid.

Its component parts.

This gives for the composition of superoxalate of potash

65·8 oxalic acid,
34·2 potash.

100.

Consequently 100 of potash combined with 192·4 of oxalic acid.

Dr. Wollaston's quadroxalate of potash.

Method of obtaining quadroxalate of potash.

This may be obtained in several ways; either by adding acid to the superoxalate; or by causing the muriatic, sulphuric, or nitric acid to act on the superoxalate; or by boiling crystals of oxalic acid in a solution of muriate of potash. What determines the separation of the quadroxalate is its being less soluble than either of the two compounds just examined.

After having purified the salt, obtained by one of these means, by a second crystallization, I dried it on a water-bath, and subjected it to the same analysis.

10 gr. urged in the fire yielded 2·7 of subcarbonate of potash = 1·895 of potash.

10 gr., brought to the neutral state by ammonia, and precipitated by muriate of lime, yielded 11·62 gr. of oxalate of lime = 7·205 gr. of oxalic acid.

Its component parts.

100 gr. of quadroxalate of potash, therefore, are composed of

18·95 of potash,
72·05 of oxalic acid,
9 of water.

100.

Hence

Hence it follows, that 100 parts of potash are combined with 381 of oxalic acid in this salt. And from the preceding analyses, compared with this, that 100 parts of potash are combined with

97·6 of acid, in the neutral oxalate,
192 ——— in the superoxalate, and
381 ——— in the quadroxalate.

Proportions of acid in the three salts.

These quantities are to each other nearly in the ratio of the numbers 1, 2, 4.

For the knowledge of this curious fact we are indebted to Dr. Wollaston*. I have repeated his experiments, and confirmed their results.

Potash will not combine with more acid.

I endeavoured by other means, to combine potash with a larger quantity of acid, but found I could not. Having evaporated a solution of quadroxalate of potash, to which I had added a very large quantity of oxalic acid, the first crystallization separated the quadroxalate; and I could obtain nothing afterward but crystals of oxalic acid free from potash†.

Oxalate of soda.

Oxalate of soda.

This is very sparingly soluble in water; in which respect it differs much from the oxalate of potash, which on the contrary dissolves very easily in this fluid.

10 gr. of crystallized oxalic acid were dissolved in water, and neutralized by soda. The oxalate was evaporated to dryness, and heated strongly in a platina crucible. The result was 8·1 gr. of subcarbonate, containing 5·064 of soda. Hence 100 parts of oxalate of soda consist of

Its component parts.

58·92 oxalic acid,
41·08 soda.
———
100.

And 100 of soda combine with 143·5 of oxalic acid.

* Bibliothèque Britannique. [Philos. Trans. for 1807, p. 95: or Journal, vol. XXI, p. 164.]

Salt of sorrel sometimes a quadroxalate.

† When I began my experiments, I had procured some salt of sorrel from a very respectable druggist; and I attempted to combine it with a larger quantity of acid by the methods indicated by Dr. Wollaston, but without success. Hence I was about to conclude, that the quadroxalate did not exist; when I discovered, that the salt on which I was operating was a real quadroxalate. This proves, that the salt of sorrel of the shops is sometimes a quadroxalate, and consequently combined with too much acid.

Superoxalate

Superoxalate of soda.

Superoxalate of soda.

The superoxalate of soda is less soluble than the neutral oxalate. It may be obtained by direct combination of the oxalic acid with soda, or by the action of oxalic acid on muriate of soda.

10 gr. of this salt, urged in the fire, yielded 4·09 gr. of subcarbonate of soda = 2·557 gr. of soda.

10 gr. of the same salt, precipitated by muriate of lime, yielded 11·741 of oxalate of lime = 7·28 gr. of oxalic acid. The proportions of this salt therefore are

Its component parts.

25·57	soda,
72·80	oxalic acid,
1·63	water.
100.	

And 100 parts of soda are combined in it with 284·7 of acid.

Its acid double that of the oxalate.

It appears by the proportions I have given, that in the acid oxalate of soda the base is combined with twice as much acid as in the neutral oxalate, analogous to what occurs with potash. In confirmation of this, I urged in the fire 10 gr. of superoxalate of soda; and the alkali, resulting from its decomposition, was sufficient exactly to neutralize 10 gr. of the same superoxalate.

No quadroxalate.

I tried in several ways to combine soda with a larger quantity of acid, but I could not succeed; so that I believe no quadroxalate of soda exists.

Oxalate of ammonia.

Oxalate of ammonia.

Mr. Berthollet has ascertained by very accurate experiments, that a solution of ammonia of the specific gravity of 0·9656 contains 8·761 of real ammonia in 100. Adopting this datum, the most convenient method of determining the proportions of the compound of ammonia with oxalic acid appeared to me to be, to find the quantity of this ammonia necessary to neutralize a given weight of oxalic acid.

5 gr. of crystallized oxalic acid required for their neutralization 9·5 of ammonia of the spec. grav. abovementioned,

ed, or 0·83 of real ammonia. We have therefore for the proportions of dry oxalate of ammonia

27·66 ammonia,
62·34 oxalic acid*.

Its component parts.

Consequently 100 of ammonia combine with 261·4 oxalic acid.

Water in the crystals.

I ascertained the quantity of water contained in crystallized oxalate of ammonia by precipitating it with muriate of lime, and found it was 13 in 100.

I likewise employed neutralization by ammonia to find the excess of acid in the superoxalates: and I found the results analogous to those I have already given.

Thus, after having found by calcination, that 10 gr. of superoxalate of potash contained 3·46 of potash; I found, that it required 1·254 of real ammonia, to bring them to the neutral state. The quantity of acid of the superoxalate of potash therefore is 3·32 gr. neutralized by the potash in it, added to 3·28 neutralized by the ammonia. This result, extremely near what I have given, proves, that the excess of acid of the superoxalate is equal to that which is neutralized by the potash in the salt.

Superoxalate of ammonia.

Superoxalate of ammonia.

The superoxalate of ammonia too is less soluble than the neutral oxalate.

10 gr. of this salt yielded 11·84 of oxalate of lime = 7·34 of oxalic acid; a quantity capable of neutralizing 2·81 of ammonia. If then the alkali in the superoxalate of ammonia were combined with twice as much acid as in the neutral oxalate, the quantity of ammonia, necessary to bring the salt in question to the neutral state, must be 1·4. Now I found it by experiment to be 1·35.

* I have given the figures here as they stand in the original; but, as there was evidently some mistake, the amount being only 90, I endeavoured to find where the errour lay. From what follows, combined with the proportion of real acid before assigned to the crystallized acid by the author, it appears, that the 5 in the beginning of the paragraph should have been a 3; and the proportions, 27·66 ammonia to 72·34 oxalic acid; or 100 ammonia to 261·53 acid. C.

We

We may therefore consider this position as true; and in this case the superoxalate of ammonia would be composed of

Its component parts.

73·4	oxalic acid,
14	ammonia,
12·6	water.
100.	

Whence it follows, that 100 of ammonia are combined with 523 of oxalic acid in this salt.

I could never succeed in the attempt to form quadroxalate of ammonia.

Oxalate of strontian.

Oxalate of strontian.

This salt is nearly insoluble in water.

10 gr. of crystallized oxalic acid were dissolved in water, and neutralized by strontian. The oxalate of strontian, being evaporated to dryness, and exposed to a strong heat in a platina crucible, left 11·9 gr. of carbonate of strontian*; which, having been decomposed by nitric acid, yielded 8·687 of strontian.

From this analysis we have for the composition of oxalate of strontian

Its component parts.

45·54	oxalic acid,
54·46	strontian.
100.	

Whence it follows, that 100 of strontian unite with 83·62 of acid.

Dr Thomson deceived in his proportions.

Dr. Thomson has certainly been deceived in his calculation respecting the oxalate of strontian. It would follow, indeed, from the numbers he gives, that barytes has a greater capacity for saturation than strontian, which is contradictory

Component parts of carbonate of strontian.

* As I have been under the necessity of repeating this experiment several times, I took great care to ascertain the proportions of carbonate of strontian. The mean of all my experiments gave

Strontian	73·6
Carbonic acid	26·4
	100.

tradictory to all the analyses of salts with base of strontian and barytes hitherto known.

On the other hand, after having ascertained, by an experiment similar to that I have just mentioned, the proportions of oxalate of strontian, he repeated it a second time by first neutralizing a given weight of oxalic acid with ammonia, and afterward precipitating by muriate of strontian: but in this way he found, that the strontian combined with twice as much acid as he found at first; whence he inferred, that the salt obtained in this process was a superoxalate of strontian, in which the strontian was combined with twice as much acid as in the neutral oxalate. Supposed he had formed superoxalate of strontian:

The little solubility of the oxalate of strontian perhaps misled Dr. Thomson: but it seems to me demonstrated, that, in precipitating neutral muriate of strontian by the neutral oxalate of ammonia, a salt with excess of acid cannot be formed, for the residuum remains neutral. In the next place, I do not think, that an acid oxalate of strontian exists; for I have not been able to form it, in employing the same means as for the other oxalates; and, besides, the neutral oxalate of strontian is very little soluble in an excess of its acid. Lastly, since the proportions he gives for the neutral oxalate are not accurate, his simple ratio between the neutral and acid oxalate is done away. but this does not exist.

Oxalate of barytes.

This salt is more soluble in water than oxalate of strontian. Oxalate of barytes.

10 gr. of the same crystallized oxalic acid were neutralized by barytes. The oxalate, urged in the fire, yielded 15·3 gr. of carbonate of barytes; which, when decomposed by sulphuric acid, left 11·934 gr. of barytes*.

* I have found by my experiments, that carbonate of barytes is composed of

Barytes	78
Acid	22
	100.

Component parts of carbonate of barytes.

These are the same proportions, as Klaproth assigns it.

From

From this analysis the elements of oxalate of barytes must be

Its component parts.

62·17 barytes,
37·83 oxalic acid.

100.

Whence it follows, that 100 of barytes combine with 60·84 of oxalic acid.

Superoxalate of barytes.

Superoxalate of barytes.

When crystals of oxalic acid are boiled in a solution of muriate of barytes, and the liquor is afterward allowed to cool, crystals are deposited, which are superoxalate of barytes. The formation of this salt was first noticed by Darracq*.

Decomposable by water.

This combination has so little stability, that boiling the salt in water is sufficient, to deprive it of all its excess of acid†.

To analyse it, I urged in the fire 10 gr.; and thus found, that they contained 4·504 of barytes.

I also boiled 10 gr. of the same salt in distilled water, which dissolved out all its [excess of] acid: and 1·102 of real ammonia were necessary, to neutralize the liquid. The acid contained in this salt therefore was

Its component parts.

2·74 saturated by the barytes in the salt,
2·80 saturated by ammonia.

5·50

Superoxalate of barytes, therefore, is composed of

55 oxalic acid
45 barytes.

100.

And 100 of barytes combine with 123 of oxalic acid.

Twice as much acid as the neutral salt.

Thus we see too, that barytes is combined with twice as much acid in the superoxalate, as in the neutral oxalate.

* Ann. de Chim. vol. XL, p. 69.
† Thomson's System of Chemistry vol. IV.

Oxalate

Oxalate of magnesia.

This salt is completely similar to oxalate of lime in many respects. I analysed it in the same manner, because the little solubility of magnesia did not admit of neutralizing a given weight of the acid by this alkali.

Oxalate of magnesia.

10 gr. of this salt, dried on a water-bath till its weight was no longer diminished by the heat, were exposed to a strong heat in a platina crucible, and yielded 2·86 gr. of magnesia, containing 0·125 of a gr. of carbonic acid.

We have therefore for the proportions of oxalate of magnesia

Its component parts.

27·35 magnesia,
72·65 oxalic acid.

100.

This gives 265·6 of oxalic acid to 100 of magnesia.

The oxalate of magnesia is extremely little soluble in water, and in an excess of its acid. Yet, when a solution of sulphate of magnesia is mixed with one of oxalate of ammonia, no precipitate is produced. Dr. Thomson, in relating this fact, seems to oppose it to the principle, that the separation of salts is determined by the force of cohesion: but I have observed, that letting the mixture stand some time is sufficient, to precipitate the oxalate of magnesia completely, without our being capable of redissolving it.

Sulphate of magnesia not immediately precipitated by oxalate of ammonia, but slowly decomposed.

Such are the proportions that result from my analyses of the oxalates. Some of them differ from those, which Dr. Thomson has given*; so that it was not till I had repeated them with all the care, of which I was capable, that I placed confidence in my results. What appears to me to confirm them is, that they agree much better with the capacities of the alkalis for saturating acids already admitted.

Comparison of the results.

For the sake of a more ready comparison, I shall here give a tubular view of my analyses and those of Dr. Thomson; and I shall add, in the last column, the propor-

* I believe, the principal difference between Dr. Thomson's analyses and mine arose from that gentleman, certainly a very expert chemist, having operated with too small quantities.

Dr. Thomson used too small quantities.

tions

tions calculated from the capacity of the alkalis for muriatic acid*, supposing, that those of oxalate of lime are accurate.

Tabulated results.

Names of the neutral salts.	Acid.	Base obtained in my experiments.	Base obtained in Dr. Thomson's	Base calculated from the capacity for saturation.
Oxalate of lime	100	61·2	60	61.2
——— of potash	100	102·7	122·86	103·8
——— of soda	100	69·7	57·14	68·9
——— of amm.	100	38·2	34·12	
——— of stron.	100	119·5	151·51	113·4
——— of baryt.	100	164·3	142·86	164·3
——— of magn.	100	37·6	35·71	†

All the oxalates do not combine with more acid.

All the oxalates have not the property of combining with an excess of acid, as my experiments show. It is the force of cohesion of the acid, combined with that of the alkali, which determines the existence of the superoxalates.

The acid has great force of cohesion,

In fact, the great number of insoluble salts, which the oxalic acid forms with the bases, tends to prove, that this acid possesses great force of cohesion. To this quality is owing its property of forming with the soluble alkalis salts with excess of acid less soluble than the neutral salts‡.

which accounts for this,

Accordingly the soluble oxalates alone can take up an excess of acid. It is true the oxalate of barytes, which is but sparingly soluble, is capable of forming a superoxalate; but the excess of acid is so feebly retained in this compound, that the action of water is sufficient to separate it.

and for the formation of quadroxalate of potash.

We may further consider it as a natural consequence of what I have just observed, that potash, which forms the most soluble superoxalate, is capable of forming a quadroxalate, while on the contrary the little solubility of the superoxalates of soda and ammonia, added to the great

* I have taken the proportions of the muriates ascertained by Mr. Rose, whose accuracy is well known. I omit however the muriate of ammonia, because he analysed the salt obtained by sublimation, in which state it contains a slight excess of acid, and, no doubt a little water.

† The agreement between the proportions in this column and those deduced from my experiments is so striking, that I feel it necessary to declare, that my experiments were finished before the calculations were made.

‡ Statique chimique, tom. I, p. 351.

capacity

capacity for saturation of these bases, prevents them from forming quadroxalates.

The conclusions, that may be drawn from the observations I have here submitted to the judgment of the class, are:

1st, That the soluble oxalates alone are capable of taking up an excess of acid, and forming salts less soluble than the neutral salts: General conclusions.

2d, That the property of forming superoxalates depends on the force of cohesion of the acid, combined with that of the alkali*.

3d, That potash is the only alkali capable of forming a quadroxalate.

4th, That, in all the superoxalates, the alkali is constantly combined with twice as much acid as in the corresponding neutral oxalate.

VII.

Observations on Acetate of Alumine: by Mr. GAY-LUSSAC†.

I Long ago remarked, that, when a solution of acetate of alumine is heated, it soon grows turbid, and lets fall a large quantity of alumine. In this there is nothing strange, and it is easily explained: but, if the acetate be allowed to cool, we shall see the precipitate gradually dissolve, and the liquid resume its transparency. If the saline solution be heated a second time, it will become turbid anew, and again transparent on cooling. I have repeated these operations twenty times following, and the results have been constantly the same. Heat precipitates acetate of alumine, which is redissolved by cooling.

Acetate of alumine made with cold saturated solutions of alum and acetate of lead, and consequently but little concentrated, became turbid at 50° cent. [122° Fahr.]. It Experiments to show this.

* By force of cohesion I mean the tendency to form insoluble compounds.

† Ann. de Chim. vol. LXXIV, p. 193.

being then filtered, and exposed to a somewhat higher temperature, a precipitate was formed again. On cooling it did not resume its transparency immediately below the point at which it lost it; it was only at a much lower temperature, that the alumine was wholly dissolved. This is owing to the coherence the earth has acquired; and it is observable, that, the longer the heat has been continued, or the higher it has been raised, the more difficultly the alumine redissolves.

Another acetate of alumine, much more concentrated than the preceding, and which was very acid, because a considerable sediment had formed in it, became likewise turbid by heat, but a little slower; and this equally resumed its transparency on cooling.

Nearly half as much thrown down by heat as by ammonia.

To determine the quantity of alumine precipitated from the acetate by heat, and which varies according to the temperature, I took two equal portions of acetate of alumine obtained by the mixture of two solutions of alum and acetate of lead made without heat. One of these portions was heated to ebullition, and immediately filtered: the other was precipitated by ammonia. Both precipitates having been washed and dried, the weight of the first was found equal to nearly half of the second.

This of importance to calico-printers.

These observations may be of great importance to calico-printers; for, to obtain mordants highly concentrated, they employ hot solutions of alum and acetate of lead. Much alumine therefore must be precipitated; and, if the mixture be filtered immediately, there will be a considerable loss. To avoid this, it should be suffered to cool completely, before it is filtered, or decanted off; and frequently stirred, that the alumine may redissolve. Without these precautions the acetate of alumine would be very acid; and this no doubt is the reason, why it is usual to add chalk to it. It is easy however, to prevent the decomposition of the acetate of alumine by heat. The addition of alum, which, as is well known, has the property of dissolving alumine, will for this reason prevent the acetate from becoming turbid. A great excess of acid would answer the same purpose as alum.

Methods of preventing it.

From the preceding observations too we may easily conceive

ceive the reasons of the copious precipitation, that sometimes takes place in solution of acetate of alumine. The precipitate retains some acid as well as that obtained by the heat of ebullition; for water dissolves a part of it, and sulphuric acid expels acetic acid from it: however, it may be completely removed by repeated washings with hot water.

The precipitation of alumine by heat, and its solution at a lower temperature, are facts interesting to the general theory of chemistry, and have very few analogous to them. If this precipitation were owing to the volatilization of acetic acid, the alumine could not redissolve by cooling: besides, we observe the same phenomena with a very acid acetate, and also in vessels hermetically closed. Since then it is not owing to the volatilization of the acid, it is clear, that it must be occasioned by the heat: which, separating the particles of alumine and acid to a greater distance, carries them beyond the sphere of their action on each other, and occasions their separation: but, if the heat be diminished, these same particles enter again within their sphere of activity, and combine. This decomposition appears to me analogous to that of a neutral solution of carbonate of potash, or of soda, by heat; with this difference only, that the carbonic acid, being separated from its base, immediately flies off on account of its elasticity, and its little solubility in water; while the acetic acid remains still in presence of the alumine, because it is not volatilized by the temperature that occasions its separation.

The precipitation is not owing to volatilization of the acid.

Attempts to account for it.

Supposed analogous to the decomposition of the neutral alkaline carbonates,

It appears to me also, that this decomposition has considerable analogy with the coagulation of albumen by heat: for, according to the explanation, which Mr. Thenard has given of this phenomenon, it is owing to the tendency water has to evaporate. Thus it happens in like manner, that the particles of water and albumen are carried by the heat out of their sphere of activity, and separate. No doubt they would combine again on cooling, in the same manner as the elements of the acetate of alumine; but water is too feeble a solvent, and the coherence the albumen has acquired too great, for the solution to take place.

and the coagulation of white of egg.

 VIII.

VIII.

METEOROLOGICAL JOURNAL.

	Wind	Pressure. Max.	Min.	Med.	Temperature. Max.	Min.	Med.	Evap.	Rain	
11th Mo.										
Nov. 7	N E	29·58	29·43	29·505	52	41	46·5	—	·07	
8	S	29·65	29·35	29·500	52	42	47·0	·06	·19	☽
9	Var.	29·67	29·34	29·505	54	47	50·5	—	·34	
10	S W	29·34	29·22	29·280	55	39	47·0	·10	·21	
11	N W	29·91	29·22	29·565	52	36	44·0	—	·03	
12	N W	29·60	29·50	29·550	54	38	46·0	·18	·22	
13	W	29·73	29·69	29·710	52	35	42·5	—		
14	S W	29·65	29·57	29·610	55	40	47·5	·12	·07	
15	W	29·57	29·49	29·530	48	36	42·0	·11	·08	
16	N W	29·66	29·49	29·575	50	41	45·5	—		●
17	S W	30·17	29·66	29·965	49	44	46·5	—		
18	N	30·25	30·17	30·210	50	45	47·5	—	·01	
19	N	30·39	30·25	30·320	53	31	42·0	·12		
20	N W	30·39	30·32	30·355	48	28	38·0	—		
21	S W	30·32	30·25	30·285	46	29	37·5	·06		
22	E	30·22	30·18	30·200	45	25	35·0	—		
23	N W	30·24	30·22	30·230	47	28	37·5	·06		☾
24	S W	30·35	30·24	30·295	46	33	39·5	—		
25	N W	30·36	30·35	30·355	50	38	44·0	·05		
26	W	30·41	30·35	30·380	47	41	44·0	—		
27	S W	30·40	30·35	30·375	44	40	42·0	·06		
28	W	30·35	30·27	30·310	48	39	43·5	—		
29	S W	30·27	30·25	30·260	47	42	44·5	—		
30	S W	30·25	30·10	30·175	50	41	45·5	·08		○
12th Mo.										
Dec. 1	S W	30·10	29·50	29·800	52	48	50·0	·13		
2	N W	29·86	29·50	29·680	52	34	43·5	·08	·15	
3	S W	29·86	29·32	29·590	50	40	45·0	·13	·04	
4	W	29·45	29·21	29·330	46	31	38·5	—		
5	N	29·96	29·74	29·850	32	22	27·0	—		
6	S W	29·74	29·56	29·650	50	28	39·0	·15		
		30·41	29·21	29·898	55	22	42·95	1·49	1·41	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES.

NOTES.

Eleventh Month. 7. A calm pleasant day. 8. Cloudy, drizzling. 9. Wind, a. m. N. W.: a dripping mist: then clear and calm: *cirrostratus*, evening: rain before nine the next morning. 10. *Nimbi* at sunset, with red haze on a brilliant twilight. 11. At sunset the clouds coloured in the E.: a *nimbus* in the W.: windy night. 13. A clear sunset beneath dense clouds. 14. Windy, S. W. a. m. 15. Clear, windy: *nimbi* at sunset to S. 18 Dripping mist. 19. Fair: *cumuli* p. m. which evaporating at sunset, a beautiful red twilight ensued, with *cirrostratus*. 20. a. m. Hoar frost and ice, the first this season: clear day, with *cirrus* clouds: at sunset, the purplish haze of the dew was conspicuous, and the twilight of a rich crimson, with converging darker streaks upon it, probably the shadows of prominent objects on the earth. 21. Hoar frost: a *stratus* in the evening. 22. Cloudy through the day in the superior atmosphere: twilight milky and luminous, with a blush of red. 23. Much *rime* on the grass, &c.: the sun emerged suddenly from the surface of a dense frozen mist, *cirri* stretching from E. to W., *cirrostrati* and *cumuli* beneath: the evening quite overcast. 24. Various modifications of cloud ending in *cumulostratus*. 25. Morning twilight red. 26. Calm: lightly clouded. 27. Overcast: a few drops p. m. 29. At sunset, a *stratus*, with a veil of superior clouds on the western sky richly coloured, the reflection from which gave considerable colour to the *stratus* itself: wind above, N. W. 30. Cloudy. The weather has been calm since the 15th inst.

Twelfth Month 1. This morning the wind rose, bringing much cloud, with a few drops of rain: the night was stormy, and the evaporation was increased near sixfold: hence the formation of so great a mass of cloud, the superior atmosphere not being in a state to take up the water. 2. Rain commenced soon after eight a. m.; about this time too the thermometer, which had been rising, began to fall; the barometer, which had been descending, to rise; and the wind, which had been S. W., to go to the N. 3. Wind, a. m. fresh at S. W.: the sky overcast, chiefly with *cirrostratus*: stormy night: a shower about one a. m., after which the wind abated. 4. Clear, windy, a. m.: various clouds p. m. 5. Snow early this morning: wind N.: evening twilight orange coloured, but with fainter horizontal streaks of cloud above it, which were also discernible at the ensuing sunrise, with *cirrostratus* beneath: windy.

RESULTS.

Barometer: highest observation 30·41 inches; lowest 29·21 inches; Mean of the period 29·898 inches.

Thermometer: highest observation 55°; lowest 22°; Mean of the period 42·95°.

Evaporation 1·49 inches. Rain on the surface of the Earth, 1·41 inches: at 43 feet elevation 0·97 inches.

Wind chiefly S. W. and N. W. The fore part of the period wet, the middle fair and tending to frost, the conclusion windy and changeable. There has been a strong tendency to the red refraction during twilight.

L. HOWARD.

PLAISTOW, *Twelfth Mo.* 9, 1811.

IX.

Observations on some of the Strata in the Neighbourhood of London, and on the Fossil Remains contained in them: by JAMES PARKINSON, *Esq., Member of the Geological Society**.

Fossil organized remains important to geology.

THE study of fossil organized remains has hitherto been directed too exclusively to the consideration of the specimens themselves; and hence has been considered rather as an appendix to botany and zoology, than as (what it really is) a very important branch of geological inquiry.

Compared with living beings.

From a comparison of fossil remains with those living or extant beings, to which they bear the closest analogy, great resemblances and striking differences are at the same time perceivable. In some instances the generic characters materially differ, but in most they very closely correspond; while the specific characters are very rarely found to agree, except when the fossil appears to have existed at, comparatively, a late period. Of man, who constitutes a genus by himself, not a single decided remain has been found in a fossil state.

Chemical analysis accounts for their preservation.

Chemical analysis has been called in to the aid of the naturalist, in order to account for the perfect state of preservation observable in remains organized with the most exquisite delicacy, and which there is every reason for supposing to have been readily decomposable in their recent state. From this investigation we learn the manner, in which these memorials of the old world, so interesting and so frail, have been preserved. Some have been impregnated with calcareous matter, others with siliceous, and others with iron or copper pyrites.

Their study should be connected with that of the strata.

But these facts, however important and interesting, cannot, when considered by themselves, add much to our knowledge respecting the formation and structure of the Earth. To derive any information of consequence from them, on these subjects, it is necessary, that their exami-

* Trans. of the Geological Society, vol. I, p. 324.

nation

nation should be connected with that of the several strata, in which they are found*.

Already have these examinations, thus carried on, taught us the following highly instructive facts. That exactly similar fossils are found in distant parts of the same stratum, not only where it traverses this island, but where it appears again on the opposite coast: that, in strata of considerable comparative depth, fossils are found, which are not discovered in any of the superincumbent beds: that some fossils, which abound in the lower, are found in diminishing numbers through several of the superincumbent, and are entirely wanting in the uppermost strata: that some fossils, occurring in considerable numbers in one stratum, become very rare in the adjacent portion of the next superincumbent stratum, and afterward are lost: that fossils of one particular genus, which exist abundantly in the lower strata, and occur in several of the superincumbent ones, are not found in the three highest strata; while one species of that genus, but which has not been found in a fossil state, exists in our present seas: and lastly, that most of the remains, which are abundant in the superior strata, are not at all found in the lower. These general facts lead us to hope, that General facts.

* This mode of conducting our inquiries was long since recommended by Mr. W. Smith, who first noticed, that *certain fossils are peculiar to and are only found lodged in particular strata*; and who first ascertained the constancy in *the order of superposition*, and *the continuity of the strata of this island*. It will appear from the following quotation, that these observations have lately also occurred to Messrs. Cuvier and Brongniart, while examining into the nature of the strata of the neighbourhood of Paris. "Cette constance dans l'ordre de superposition des couches les "plus minces, et sur une étendue de 12 myriamètres au moins, est, "selon nous, un des faits les plus remarquables que nous ayons constatés "dans la suite de nos recherches. Il doit en résulter pour les arts et "pour la géologie des conséquences d'autant plus intéressantes, qu'elles "sont plus sûres.

"Le moyen que nous avons employé pour reconnoitre au milieu d'un "si grand nombre de lits calcaires, un lit dejà observé dans un canton "très-éloigne, est pris de la nature des fossiles renfermés dans chaque "couche, ces fossiles sont toujours généralement les mêmes dans les "couches correspondantes, et présentent des differences d'espèces assez "notables d'un système des couches à un autre système. C'est un signe "de reconnoissance qui jusqu'à présent ne nous a pas trompés." Annales du Muséum d'histoire naturelle, tome XI, p. 307.

geology

geology may derive considerable assistance, from an examination of fossils, made in connexion with that of the strata to which they belong.

Strata in the vicinity of London.

The following is an attempt to investigate on this plan some of the upper strata in the vicinity of the metropolis with their contained fossils; and, although by no means complete, it will, it is hoped, induce others, who possess superior abilities and opportunities, not only to reexamine more correctly these strata, but to extend their researches to the subjacent strata.

The British strata have been considerably disturbed,

The whole of this island displays evident marks of its stratification having, since its completion, suffered considerable disturbance, from some prodigious and mysterious power. By this power all the known strata, to the greatest depths that have been explored, have been more or less broken and displaced; and in some parts have been so lifted, that some of the lowest of these have been raised to the surface; while portions of others, to a very considerable depth and extent, have been entirely carried away*. From these circumstances great difficulties and confusion frequently

but least near the metropolis.

arise in examining the superior strata: the counties however immediately surrounding the metropolis, as well as that on which it stands, having suffered least disturbance, are those in which an investigation of these strata may be carried on with the smallest chance of mistake.

Real alluvial fossils rarely seen near London.

Real alluvial fossils, washed out of lifted or original superior strata by strong currents, and which in other parts are very abundant, are rarely seen in the counties adjacent to the metropolis. This remark is rendered necessary, since

Its beds of sand and gravel not alluvial.

those widely extended beds of sand and gravel, with sandy clay, sometimes intermixed and sometimes interposed, and which have been generally hitherto considered as alluvial beds, are here assumed to be the last or newest strata of this island, slowly deposited by a preexistent ocean; with the

* See several essays on this subject in the Philosophical Magazine, by Mr. Farey, and the Report on Derbyshire, vol I, p. 105.

Also a Letter on the alterations, which have taken place in the structure of rocks, on the surface of the basaltic country in the counties of Derry and Antrim, by William Richardson, D D. Phil. Trans. 1808: or Journal, vol. XXII, p. 161, 245.

the strata, therefore, of this formation, these remarks commence.

Beds of Sand and Gravel.

The sands of this formation vary in colour from white, which is most rare, through different shades of yellow up to orange-red: the colour proceeding partly from a ferruginous stain on the suface of the particles of sand, and partly from the intermixture of yellow oxide of iron. Particles of those sands, which are disposed in distinct seams or beds, when examined by the microscope, are found to be transparent, most of them angular, but some a little rounded, with all their surfaces smooth, having no appearance of fracture, and resembling, in every respect, a uniform crystalline deposition. Those sands on the contrary, which, blended with broken and unbroken pebbles, form gravel, appear, when thus examined, to be mostly opaque, to be variously coloured, and to be marked with conchoidal depressions and eminences, the result of fracture. Beds of sand and gravel.

The pebbles of this formation appear to be of four kinds, Pebbles of this formation.

1st. Various pieces of jasper, gritstone, white semitransparent quartz, and other rocks. These have acquired, in general, smooth surfaces and roundish forms, evidently from attrition, and exhibit no traces of organization, except when, as is very rarely the case, the substance of the pebble is jasperised wood. The white quartz pebbles, like quartz crystals, on being rubbed together, emit a strong white lambent light, with a red fiery streak on the line of collision, and an odour which much resembles that of the electric aura. 1st kind. Its organic remains rare, and only wood.

2d. Oval, or roundish, and rather flat siliceous pebbles, generally surrounded by a crust or coat differing in colour and degree of transparency from the internal substance, which also varies in different specimens, in these respects, as well as in the disposition of the parts of which the substance is composed. In some this is spotted, or clouded, in very beautiful forms; in others it is marked by concentric striæ, as if the result of the successive application of distinct laminæ: the prevailing colours in most of these pebbles being different shades of yellow. In several the traces of marine remains are observable: these are, in some, the casts of *anomiæ*, 2d kind. Marine remains found in several.

anomiæ, and the impressions of the spines and plates of *echini*; and in others, which generally possess a degree of transparency, the remains of *alcyonia*. The impressions, though frequently on the surface of the pebble, seldom, if ever, appear to be in the least rubbed down; thus seeming to prove decidedly, that these pebbles have not been rounded by rolling; but that they owe their figures to the circumstances under which they were originally formed: it is apprehended, therefore, that these pebbles have each been produced by a distinct chemical formation, which, it may be safely concluded from the remains of marine animals so frequently found in them, took place at the bottom of the sea, while these animals were yet living.

The pebbles not rounded by rolling.

Pebbles of similar characters found together.

The formation of these fossils at the bottom of a former sea, and perhaps on the identical spots, in which they are now frequently found, is more plainly evinced by pebbles agreeing in some peculiar characters being found together in particular spots. Thus those in the county of Essex, ten miles northward of London, contain a much greater proportion of argil and iron, than those met with in many other places; hence their colours are darker, and the delineations, which their sections display, are very strong and decided, sometimes closely agreeing with those seen in the Egyptian pebble*. Passing on into Hertfordshire, pebbles of a very different character are found; their crust is nearly black, and their section displays delicate tints of blue, red, and yellow, disposed on a dead-white ground in very beautiful forms. In another part of the same county, occurs the pebble of the pudding-stone. which also presents peculiar characters of colour, &c.

3d kind. Flints, different from those in chalk.

3d. Large tuberous, or rather ramose, irregularly formed flints, somewhat resembling in figure the flints which are found in chalk, materially differing however from them, not only in the colour of their external coat, which is of various shapes of brown; but also in that of their sub-

* The gravel pebbles of Epping Forest are of this description; and on most of the grounds leading down from the forest to the hamlet of Sewardstone, and to the town of Waltham, white, opaque, and partly decomposed pebbles are frequently seen, in which the argil and iron have been removed, and the silex only has remained.

stance,

stance, which is seldom black, but exhibits shades of yellow or brown, in which red likewise is sometimes perceptible. The traces of organic structure, particularly of the *alcyonium*, occasionally seen in these stones, determine them also to have been formed at the bottom of the sea. Organic traces in them.

4th. Pebbles, owing their form to an investment and impregnation with silex, of various marine animals of unknown genera, but bearing a close affinity to the *alcyonia*. These stones display, in general, not only the external form, but the internal structure also of these animals. The congregation of many pebbles of this genus, and indeed of the same species, in particular tracts, warrants the conclusion, that these animal substances were thus changed, while inhabiting the bottom of a former ocean, which now forms the stratum, the contents of which are here sketched. Pebbles of this description are most frequently found in the gravel pits of Hackney, Islington, &c. 4th kind. Marine animals impregnated or invested with silex, while at the bottom of the sea.

Among the traces of organization discoverable in this stratum are casts of *echini*, which are frequently found among the gravel, and which have generally been supposed to have been washed out of the chalk. But these casts have their origin plainly stamped on them. Their substance is covered with iron; they are almost always of a rude and distorted form; and I apprehend, that they are never found with any part of the crust of the animal converted into spar adherent to them, as is commonly the case with the casts of echini found in chalk. Casts of echini, not washed out of chalk.

A sufficient proof, that these several strata of gravel, sand, &c. have been deposited by a former ocean, is to be found in a circumstance, which does not appear to have been hitherto sufficiently adverted to. This circumstance is the existence of fossil shells belonging to, and accompanying, the superior part of these strata in particular spots: their absence in other parts being, perhaps, attributable to the removal of the upper beds. Fossil shells accompanying these strata,

These fossil shells are still found disposed over a very considerable extent. Their nearest situation to the metropolis is at Walton Nase, a point of land about sixteen miles S. E. of Colchester. Here a cliff rises more than fifty feet above high water mark and the adjacent marshes. It is formed disposed over a considerable extent now. At Walton Nase,

formed of about two feet of vegetable mould, twenty or thirty feet of shells, mixed with sand and gravel, and from ten to fifteen feet of blue clay. The bed of shells is here exposed for about three hundred paces in length, and about a hundred feet in breadth.

Harwich cliff,

Immediately beyond the Nase the shore suddenly recedes and forms a kind of estuary, terminated towards the east by the projecting cliff of Harwich, which is capped in a similar manner with beds of these shells. The height of this cliff is from forty to fifty feet, about twenty-two feet of the lower part of which is the upper part of the blue clay stratum; " above which", as Mr. Dale observes, " to within " two feet of the surface, are divers strata of sand and " gravel mixed with fragments of shells, and small peb- " bles; and it is in some of the last mentioned strata, that " the fossil shells are imbedded. * These fossils lie promis- " cuously together, bivalve and turbinate, neither do the " strata in which they lie observe any order, being some- " times higher and sometimes lower in the cliff; with strata " of sand, gravel, and fragments of shells between. Nor " do the shells always lie separate or distinct in the strata, " but are sometimes found in lumps or masses, something " friable, cemented together with sand and fragments, of a " ferruginous or rusty colour, of which all these strata are*."

and through Suffolk and great part of Norfolk.

The coast of Essex is here separated from that of Suffolk by the river Stour, by which the continuity of this stratum is necessarily interrupted. It however occurs again on the opposite side of the river, and through Suffolk and great part of Norfolk the same bed of shells is found on digging; thus appearing to extend over a tract of at least forty miles in length.

Sometimes they are mixed, sometimes the species separated.

These shells are in general found in the same confused mixture, as is described by Mr. Dale; but they are also sometimes so disposed, that patches of particular genera and species appear to be occupying the very spots where they had lived. This seems particularly the case with the small *pectens*, the *mactræ*, and the *left-turned whelk*.

* Appendix by Samuel Dale to the History and Antiquities of Harwich and Dovercourt by Silas Taylor, 1732.

From

From the excellent state of preservation, in which many of these shells have been found, it has been thought, that they could hardly be regarded as fossil. Many acknowledged fossil shells however have undergone much less changes than those of this stratum; the original coloured markings are entirely discharged, and the external surfaces are deeply penetrated with a strong ferruginous stain; the inner surfaces also are considerably changed, their resplendence being superseded, to a considerable depth, by a dead whiteness, the consequence of the decomposition of this part of the shell. In excellent preservation.

Like the fossils of most other strata this assemblage of shells manifests a peculiar distinctive character. A few shells only, which may be placed among those which are supposed to be lost, or among those which are the inhabitants of distant seas, are here discoverable; the greater number appearing not to differ specifically, as far as their altered state will allow of determining, from the recent shells of the neighbouring sea. Mostly do not differ specifically from those of the neighbouring sea.

Among those, of which no recent analogue is known, appears to be the *terebratula*, figured in Dale's History and Antiquities of Harwich, &c. tab. XI, fig. 9, p. 294, and described, Phil. Trans. No. 291, p. 1578. Mr. Dale describes this shell as *Concha longa fossilis fasciata*, and remarks, that he has not observed "either in Aldrovandus, "Rondeletius, Belonius, Gesner, Johnson, Lister, or "Bonanus, any shell, that resembles this our fossil, unless "it is one of those figured by Lachmund, p. 43, No. 6 "and 7, the inward part resembling our fossil." The shells figured by Lachmund are undoubtedly *terebratulæ*, but they manifest no particular agreement with this fossil. A shell resembling no recent one known.

This shell appears to be figured by Lister, *Histor. Conchyl. tab.* 211, *fig.* 45, and is assumed by Gmelin, as *anomia spondylodes*. The other shells, fig. 46, of the same plate, referred to by Gmelin as *anomia psittacea*, appear to be mutilated specimens of the same shell. This opinion is corroborated, by the tint given by the accurate artists to the whole of the shells contained in this plate agreeing with the dark colour of the Essex fossil; and by the circumstance

stance of their being generally found in the mutilated state, in which they are here figured by Lister. Besides, neither of Lister's specimens at all agrees with the pellucid shell, with a triangular foramen, of *anomia psittacea*; but they all agree with the oval antiquated shell, with an obtuse canaliculated beak, of *anomia spondylodes*.

Terebratula spondylodes.

In consequence of this agreement, it seems proper to consider this fossil shell as forming the species *anomia spondylodes*. But as the channelled beak is not natural to it, but is the consequence of injury; and as this part, in its natural state, is pierced with a large round foramen, a correspondent change should be made in the description, and it may be placed under the more appropriate genus of terebratula, as *terebratula spondylodes*, with an oval antiquated shell, the beak pierced by a large round foramen.

This shell is, in general, about an inch and a half long, thick, nearly oval, roughly striated transversely, and has its large foramen defined by a distinct border. It appears to differ from every known recent or fossil *terebratula*.

Another lost shell, ostrea deformis.

Another of the probably lost shells of this stratum is the fossil *oyster*, figured Organic Remains, &c., vol. III, pl. XIV, fig. 3; and which is there conjectured to be the same oyster, as that which is described by Lamarck as *ostrea deformis*.

Another, a volute.

The *volute*, Organic Remains, vol. III, pl. V, fig. 13, is another shell belonging to this stratum, of which it is believed that no recent analogue has been yet found. This ovate and rather fusiform shell appears to have been smooth; and at its full size about four inches in length; the columella has four folds, and the shell is formed by about six spiral turns, the last of which makes two thirds of the shell, dilating at about its centre, and contracting nearly equally upwards and downwards. The speciumens yet seen give no opportunity of judging of the lip, or of the termination of the spire.

Reversed whelk.

The Essex *reversed whelk*, as it has been termed, *murex contrarius*. Linn. *Hist. Conch.* of Lister, *tab.* 950, *fig.* 44, *b, c*, which is here very abundant, does not appear to be known in any other stratum of the island. The fossil shell, with

with the whirls in the ordinary direction, is sometimes found in this stratum*.

It has been said, that the recent analogues of both these shells are found in the adjoining sea. A recent shell is indeed found, which very nearly agrees with the ordinarily turned shell in its general characters: but there appears no authority for supposing, that the analogue of the left-turned variety has been discovered there.

Among those recent shells, the resemblance of which to the fossil ones of this stratum is such as appears to render a comparison by an experienced conchologist necessary, may be enumerated: Fossil shells of this stratum to be compared with recent ones.

Patella ungarica, patella militaris, patella sinensis, (calyptræa, Lam.) *patella fissura, (emarginula,* Lam.) one or two species of *patellæ*, with a perforation in the apex, *(fissurella,* Lam.) *nerita glaucina, nerita canrena, (natica,* Lam.) *turbo terebra, (turritella,* Lam.) *murex corneus, murex erinaceus, strombus pes pelicani, cypræa pediculus*, with no sulcus along the back, *pholas crispatus*, in fragments, *solen ensis*, and *solen siliqua*, in fragments, *cardium edule, cardium aculeatum?* bearing the size and form of this shell, but having from thirty-four to thirty-six ribs, with no depressed line down their middle, nor vestiges of spines; *mactra solida, venus exoleta, venus scotica? venericardia senilis,* Lam., *arca glycemeris, arca nucleus.*

Beside these remains of marine animals, the fossil hollow tubercles, having lost the spines, of the *thornback* are here found; also fragments of the *fossil palate, (scopula littoralis* of Lhwydd) and fossil remains of *sponge* and *alcyonia*, particularly a very fair specimen of the *reticulated alcyonium*. Org. Rem., vol. II, pl. IX, fig. 9. Other marine fossils.

In this bed, among the gravel and the shells, are frequently found fragments of *fossil bone*, which possess some striking peculiarities. They are seldom more than half an inch in thickness, two inches in width, and twelve in length; always having this flat form, and generally marked with small dents or depressions. Their colour, which is brown, Peculiar fragments of fossil bone

* It is erroneously stated, Organic Remains, vol. III, p. 66, that this shell has not been yet mentioned, as found in this stratum; since it is so particularised by Dale.

light

light or dark, and sometimes inclining to a greenish tint, is evidently derived from an impregnation with iron. From this impregnation they have also received a great increase of weight and solidity; from having been rolled they have acquired a considerable polish; and on being struck by any hard body they give a shrill ringing sound. These fragments, washed out of the stratum in which they had been imbedded, are found on the beach at Walton, but occur in much greater quantity at Harwich.

Part of a tooth of a mammoth.

Of the flat rounded pieces described above, no conjecture can be formed as to the particular bone, or particular animal, to which they belonged. But within these few years an Essex gentleman found, on the beach at Harwich, a tooth, which was supposed to have belonged to the *mammoth*. This fossil was kindly obtained at my request, for the purpose of being exhibited to the members of the Geological Society, by my late friend Dr. Menish; and certainly it appeared to be part of a tooth of that animal. It had been broken and rounded by rolling, but its characters were still capable of being ascertained. It possessed, in the softer parts, the colour and appearance of the Essex mineralised bones so distinctly, as to leave not a doubt of its having been imbedded in this stratum; while in the enamel it manifested decided characters of the tooth of some species of the mammoth, or *mastodon* of Cuvier.

Extent of this stratum.

The actual limit of this stratum has not been ascertained; it is however known to extend through Essex, Middlesex, part of Kent, and Surry, and through Hertfordshire, Buckinghamshire, and indeed much farther both to the northward and westward. In many parts its continuity has been interrupted, apparently by partial abruptions of it, together even with a portion of the stratum on which it rests. The shells of this stratum have hitherto been discovered only in the parts already noticed.

Blue clay stratum.

Blue clay stratum.

This, the next subjacent bed, is formed of a ferruginous clay exceeding two hundred feet in thickness. Its colour for a few feet in the upper part is a yellowish brown, but through the whole of its remaining depth is of a dark bluish gray,

gray, verging on black. It is not only characterised by these circumstances, but by the numerous septaria, which are dispersed through it, and by the peculiar fossils, which it contains.

Cause of the difference in its colour.

The difference of colour observed between its superior and inferior part, and which has generally been supposed to be owing to a difference in the degree of oxidation of the iron present in it, appears to be the result of a difference in the quantity of it, occasioned by the washing away of this metal in the upper part by the water which percolates through it, and which runs off laterally by the numerous drains made near the surface. The dark red colour of tiles made from the blue clay, the reddish-yellow colour of the *place* bricks made of the yellowish-brown clay, and the bright yellow hue of the *washed malms*, those bricks which are formed of the yellow clay which has been exposed to repeated washings, are thus accounted for.

Septaria.

The septaria lie horizontally, and are disposed at unequal distances from each other in seemingly regular layers; and, as has been just observed of the stratum itself, they become of a paler colour, and it may be added suffer decomposition, when placed so high in the stratum, as to be exposed to the action of percolating water. They frequently include portions of wood pierced by the *teredines*, *nautili*, and other shells; and it is a fact, that may be worthy of being attended to, while inquiring into their formation, that the septa of calcareous spar frequently intersect the substances enclosed in the septaria.

Their septa frequently intersect the substances enclosed in them.

Extent of this stratum.

This stratum is to be found not only wherever the preceding deposition extends, but in other parts also, where that has been removed. The cliffs of this clay, at Shepey, extend about six miles in length; the more elevated parts, which are about ninety feet in height, being about four miles in length, and declining gradually as they terminate towards the east and west.

Its fossils, in Shepey,

The fossils of this stratum have been already carefully particularised. A catalogue of those found at Shepey was added by Mr. Jacobs to his *Plantæ Favershamienses*: and an account of several of the fossil fruits found at Shepey was published by Dr. Parsons in the fiftieth volume of the Philosophical

and in Hampshire. losophical Transactions. The fossils of Hampshire have been scientifically described by Dr. Solander, in the *Fossilia Hantonensia* of Mr. Brander, where the fossils themselves are very exactly figured.

It was not supposed, even after the publication of these accounts, that the fossils of Shepey and those of Hampshire were of the same stratum. Among the Hampshire fossils no mention is made of *crabs, lobsters, tortoises, nautili*, or of the heads or bodies of *fishes* so abundant at Shepey; while the *murex pyrus, murex longævus, strombus amplus*, &c., of the Hampshire cliff had never, perhaps, been enumerated among the Shepey fossils.

The stratum in both places identical. The identity of the stratum at Shepey and in Hampshire has, within a few years, been decided by digging into this same stratum at Kew, where several of the fossils, which had hitherto been supposed peculiar to Shepey, were found in the same pit with those which had been considered as peculiar to Hampshire.

Farther proof of it at Highgate. In the present year, on cutting through a mound of this stratum which forms Highgate Hill, this identity has been still farther manifested by the discovery of great numbers of those fossils mingled together, which had been generally distinguished into Hampshire and Shepey fossils; as *crabs, nautili*, &c., like those of Shepey, together with several shells, which had been, generally regarded as peculiar to Hampshire, and in particular that uncommon alated shell, *strombus amplus*, Solander. (*rostellaria macroptera*, Lamarck.)

Certain organic remains peculiar to particular depositions. In examining this stratum, the curious fact, that certain organic remains are peculiar to particular depositions, is first observed. Very few indeed of the fossil shells of the gravel strata are to be found in the bed of blue clay. In the gravel strata, by far the greater number of the shells bear a close agreement with those, which now exist in not very distant seas; but in this clay stratum, "very few of the shells are known "to be natives of our own, or indeed any of the European "shores; but the far greater part of them, upon a comparison with the recent, are wholly unknown to us*."

* Fossilia Hantoniensia, p. 5.

But

But although this clay stratum contains fossils of a much older date than those of the gravel stratum, it possesses other marks, which agree with its position in showing, that it is of comparatively modern formation. It includes none of the remains of any of the lost fossils, such as the *cornu ammonis*, *encrinites*, &c. Mr. Jacobs indeed speaks of one imperfect specimen of *belemnites* and of *astroitæ* having been found, but at the same time as being very uncommon; Mr. Brander however does not appear to have met with any of these older fossils; nor have any of them been discovered either at Kew or at Highgate. Hence it seems reasonable to conclude, that the single imperfect belemnite and the few astroitæ were not inhabitants of the sea at the period when this stratum was deposited, but were washed out of some of the more ancient strata, and lodged by accident in the bed where they were found*.

This stratum, though older than the former, yet modern.

No remains of lost fossils in it.

The quantity of fruit or ligneous seed vessels and berries, which has been found in this stratum at Shepey, is prodigious. Mr. Francis Crow, of Feversham, has procured from this fertile spot a very large collection; and, by carefully comparing each individual specimen by their internal as well as their external appearance, he has been enabled to select seven hundred specimens, none of which are duplicates, and very few agree with any known seed vessels. These vegetable remains have also been found on the opposite Essex shore, but in very small numbers. They have also been met with in that part of the stratum, which has been examined at Kew. At Highgate and at Shepey a resinous matter, highly inflammable, of a darkish brown colour, and yielding, on friction, a peculiar odour, has also been found. This substance has been conjectured, to exist in an unaltered state; and this indeed seems to be the fact from its resinous fracture: but it must be observed, on the other

700 specimens of seed vessels.

Peculiar resinous matter.

* It appears to be necessary to guard against two sources of errour, while appropriating fossils to their respective strata: one is the circumstance here alluded to, where the fossils of a preexistent stratum have been washed out by the waters while depositing a more recent stratum: the other is where, at the line of junction of two strata, the animals of the one are found within the borders of the other stratum; a circumstance by no means difficult to be conceived or explained.

Two sources of errour.

 hand,

hand, that pieces of it occur, which are penetrated by iron pyrites.

Land animals appear to have resided on it.

This stratum is also rendered exceedingly interesting by its surface appearing to have been the residence of land animals, not a single vestige of which seems to have been found in any of the numerous subjacent strata of the British series. Mr. Jacobs relates, that the remains of an *elephant* were found at Shepey. The remains of the *elephant*, *stag*, and *hippopotamus* have also been dug up at Kew. At Walton, in Essex, not only the remains of the *elephant*, *stag*, and *hippopotamus* have been discovered, but also remains of the *rhinoceros*, and of the *Irish fossil elk*. Org. Rem., vol. iii, p. 366.

Situation of their remains.

It has been generally supposed, that these remains were contained within the stratum of blue clay; but the circumstances, under which they are found, seem rather to warrant the conclusion, that they were deposited on the surface of those low spots, where abruptions of the superior part of this stratum had taken place. Thus the remains of the elephant mentioned by Mr. Jacobs were not in the cliff, but in a low situation at a distance from it; so also the remains of land animals in Essex occur a little below the surface, in a line with the marshes, which are a very few feet above high water mark. By a communication of the late Mr. William Trimmer, of Kew, it appeared, that he found, under the sandy gravel, a bed of earth, highly calcareous, from one foot to nine feet in thickness; beneath this a bed of gravel a few feet thick, containing water; and then the main stratum of blue clay. At the bottom of the sandy gravel, he observed, that the bones of the *hippopotamus*, *deer*, and *elephant* were met with; but not in those parts of the field, to which the calcareous bed did not extend. Here also a considerable number of small and apparently fresh-water shells, and, at the bottom, snail-shells were found. Does it not seem, that the first appearance, or creation, of land-animals was on the dry land of this stratum; and that they were overwhelmed in these spots, by that sea, which deposited the present superincumbent strata of gravel?

Shells found there.

Formation and destruction of these animals.

(To be concluded in our next.)

X.

X.

Various Observations respecting the Art of Glassmaking, with a View to explain some Phenomena, that occur in the Fabrication of Glass, and point out the Application of these to the obtaining of new Products: *by Mr.* GUYTON-MORVEAU*.

THE art of glassmaking, though one of the most ancient, since there are documents that attest its having been practised by the Pheuicians, was long, like most of the useful arts, nothing more than a tradition of those processes, which had most uniformly succeeded. Now, however, we feel the necessity of combining with it those principles, the application of which has successively unfolded the essential circumstances of the processes, increased and improved their products, and may yet afford new views either of economy or perfection.

Art of glassmaking empirical,

but ought not to continue so.

Such was the object Mr. Loysel proposed to himself in 1791, in the Essay presented by him to the Academy of Sciences; which, under that modest title, left far behind it the works of Néry, Merret, Kunckel, Haudiquert, Blancourt, and others, who had written on the subject.

Writers on it.

Still more recently the labours of Mr. d'Artigues† have given us hopes of a treatise, that would embrace the whole of this art, and place it on a footing with the present state of our knowledge.

D'Artigues.

The two papers, that Mr. d'Artigues has already communicated to the class, have called my attention to some facts, that I had noted down; and which appear to me sufficiently connected with the most important phenomena in the processes of this art, not to be consigned to oblivion. Of these I shall proceed to give a succinct account, with such reflections as may tend to elucidate their theory.

Facts noticed by the author.

The chief subjects of these observations will be:

1. The separation of glass of different densities by eliquation:

Classification of these.

* Ann. de Chim. vol. LXXIII, p. 113. Read to the Institute, the 29th of Jan. and 5th of Feb 1810.

† See Journal, vol. X, p. p. 58, 89.

2. The

2. The results of the annealing of large masses in crucible-moulds:

3. The colouring of glass red by copper, and in cements:

4. The alteration glass undergoes by long-continued heat:

5. This alteration by the fire of our furnaces compared with that of volcanoes:

6. Lastly, what constitutes the real difference between transparent and devitrified glass.

Obs. I. *Separation of glass of different densities by eliquation.*

Experiments of Buffon.

In 1776 I accompanied Mr. de Buffon to the plate glass manufactory then existing at Rouelle, near Langres, under the direction of Mr. Allut, who wrote the article *glacerie* in the *Encyclopédie*. His object was to make some experiments on the fabrication of a mass of flint-glass, for constructing the *lentille à échelons* described in the first volume of his Supplements. I shall not speak of the various processes tried, and the difficulties that obliged him to give up the hope of obtaining one single piece of sufficient thickness; but confine myself to the very extraordinary result of one trial I witnessed, and which I conceive may be compared to what metallurgists term *eliquation*.

Glass separated into two small strata as if by eliquation.

A mass of flint-glass*, 37 mil. [1·46 inches] thick, had just been run out on the copper table. A portion of this glass, about three or four fingers thick, was left in the crucible; and it was supposed, that, by charging it afresh with the common composition, the glass obtained would be so much the finer, because it would approach nearer in quality to flint-glass. The refined glass having been ladled into the cistern, and run on the table to the thickness of three lines, was placed in the annealing furnace. When it was taken out, its quality was examined; and, to our great surprise, on cutting it there appeared, instead of a single glass, two very distinct strata, the line of separation of which was plainly marked, and extended throughout the mass; the lower stratum occupying about one third of the

* The composition was 32 parts powdered Madagascar crystal, 32 minium, 16 soda, and 1 nitre. Elém, de Chim. de Dijon, vol, I, p. 179.

thickness,

thickness. I brought away a piece of it, which I showed at the public lectures of the Academy of Dijon, in the collection belonging to which it was deposited.

It was already well known, that glass, in the composition of which a large proportion of oxide of lead is employed, does not easily afford a homogeneous mass, because the denser parts are not retained by an affinity capable of producing an equilibrium; and hence the difficulty of obtaining flint-glass free from streaks. But such a speedy and complete precipitation is a solitary instance, arising from a combination of circumstances, which we can scarcely hope to reproduce. The lead renders flint-glass seldom uniform.

From what has been said, we can scarcely doubt, that the streaks, from which glass abounding in oxide of lead is seldom free, arise from a commencement of eliquation. The horizontal position of these streaks proves it; for they are not distinctly perceptible, except the light comes to the eye in a direction parallel to the zones of unequal density. I have a piece of flint-glass, manufactured also under my own eye, which is three cent. [1·18 in.] thick, which any one, not apprised of the contrary, would suppose to be perfectly good, because the division is softened down. Its streaks a commencement of eliquation.

Obs. II. *Trials of a crucible-mould for annealing large masses of glass.*

In the various experiments made at the plate-glass manufactory at Rouelle for the same purpose, a hard calcareous stone, cut in the shape of a circular crucible, was at first employed. It was supposed, that, when it had been converted into lime by a graduated fire, without having its shape altered, it would hold the refined glass; so that, bounding the bottom accurately, the glass would be annealed by cooling slowly in it, as in an annealing furnace. The result was a mass full of large blebs throughout its substance, and on its surface. Stone mould for annealing large masses of glass. Effects.

With the same view trial was made of a crucible-mould made of the best potter's clay, and baked as hard as possible. The glass was perfectly refined in this, and retained its homogeneousness in annealing; but the mass, 7 cent. Pottery-mould. Result.

[2·75

[2·75 in.] thick, and 120 [47·2 in.] in diameter, was divided by cracks from the centre to the circumference; because the adhesion of the glass to the sides of the crucible had prevented it from contracting its dimensions, the clay, so highly baked, not being susceptible of an equal diminution of bulk in cooling. I preserve a piece of this, cut in the form of a *serre-papier*, the transparency of which in so great a thickness is remarkable, though the composition was not prepared for being colourless.

OBS. III. *Glass coloured red by copper.*

Red glass. Hitherto glass has been stained red, whether for church windows, or for imitating gems, only by combining in different proportions, according to the tint desired, oxide of gold by tin, oxide of manganese, and sulphuret of antimony. Such are the compositions indicated by Fontanieu, and in Loysel's Essay on Glassmaking.

Glass stained red by iron, Clouet has given a different process in his Inquiries into the Composition of Enamels, which he was so kind as to communicate to me in manuscript some years ago, and which I published in the Annals of Chemistry, for May, 1800. This process consists in fixing the colour of red oxide of iron, by calcining a mixture of sulphate of iron and sulphate of alumine: but he precisely declares, that we have no metallic oxide, which gives a red directly by fusion ······ that this colour must be composed of different substances······and that it is desirable to multiply experiments with the new metals, which would perhaps furnish a red, *that is not to be produced directly or easily by any of the metallic substances anciently known.* He speaks of the oxide of copper only in the preparation of green enamel: and and by copper. though he sometimes obtained a tolerably fine red from it, particularly by mixing with it oxide of iron, he says, that this colour is very fugacious, and frequently disappears even while the glass is making.

Red glass from copper by an accident An accident, that happened in 1783 at the plate glass manufactory of St. Gobin, appeared to me to ascertain the circumstances, in which we may hope to fix in glass the colour of red oxide of copper; and a direct experiment, made

made at the laboratory of the Polytechnic school, tends to confirm this conjecture.

It is the practice in plate glass manufactories, when the glass is refined, to ladle it out of the pot into a cistern, which is afterward taken out of the furnace, that the glass may be run on a table. The ladles are made of copper, with an iron handle, and are dipped into water, as soon as they begin to get hot. A workman, having neglected this precaution, brought out only a part of his ladle. It was supposed, that the melted portion would sink to the bottom of the pot, and be preserved there as under a vitreous flux. Accordingly the casting and annealing were proceeded with as usual; but to the surprise of the workmen, the glass exhibited, not only a few metallic grains embedded in it, but bands pretty uniformly coloured of a very bright red. I lay before the Class a piece of this glass, polished on one side, 17 cent, [6·7 in.] long, 12 cent. [4·7 in.] wide, and 7 mil. [2·75 lines] thick. Part of a ladle being melted in plate glass.

There can be no doubt, that this colour was produced by the copper carried suddenly to that degree of oxidation, which gives it this property; and fixed in this state by its diffusion through the vitreous mass. But can we be certain of reproducing the same circumstances? and by what means? This I was desirous of ascertaining by experiment. Theory. Can we produce the effect at pleasure?

I took some powdered plate glass, mixed it with three per cent of copper filings, and brought the mixture to complete fusion. The glass was without colour, and the copper in metallic globules. Experiment with plate glass,

I repeated the experiment with common white glass and six per cent of copper filings; and obtained a vitreous mass, well fused, and of a very uniform red colour, but so deep, that it appeared in the state of enamel rather than of glass. On the surface was observable a crust less compact, approaching to the nature of scoriæ, of a brown inclining to black. with common glass,

Mixtures of glass and copper in the state of oxide, even in the lowest degree, afforded only a greenish tinge, and part of the copper was reduced. and with oxide of copper.

These results, while they announce the possibility of producing

producing a red glass with copper, confirm the opinion of Clouet respecting the difficulty of rendering this colour fixed in the fire. But why did plate glass afford only reduced copper, while common white glass produced a vitreous oxide? It seems to me, that it would be difficult to account for it by supposing, that the latter contained some oxigenating substance: but it offers itself naturally when we consider, that the composition of the former, being much more fusible, occasioned the fusion of the metal, and thus withdrew it from the action of the air, before the temperature was sufficiently high to be effectual.

Attempt to account for the difference.

It is unnecessary to observe, that this explanation is not inconsistent with the phenomenon before described; since the spoon did not pass to the state of vitreous oxide in the plate glass, till it had repeatedly undergone the action of the air and of the heat of the furnace simultaneously.

Attempts to colour glass with metallic cements.

Mr. d' Arcet has made several trials for colouring glass with cements impregnated with colouring metallic oxides. He employed iron, copper, cobalt, and manganese, in various proportions, and in different states. Iron left but a pale colour. Cobalt and manganese coloured only the cements. In that made with copper left from the distillation of its acetate, the glass was completely devitrified, and of a deep green at its surface, the colour growing lighter toward the centre, where it had a reddish tinge. A plate of glass coloured by cobalt having been placed in the common cement with a capsule of white glass, and exposed to a heat of 50° of Wedgwood; part of the capsule was found to be tinged blue, without having undergone fusion, the surfaces being only divested of their polish, and a little roughened; which is readily accounted for by the known property of this metallic oxide to rise in vapour at a very high temperature.

Obs. IV. *Of the alteration that glass undergoes by the action of great heat long continued.*

Devitrification of glass

The interesting paper of Mr. d'Artigues on the devitrification of glass* has turned men's opinions toward the

* An. de Chim. vol. L, p. 325: or Journal, vol. X, pp. 58, 89.

real

real cause of this phenomenon, which was too long considered as the product of a cementation according to Reaumur's process*. Certain facts, which I noticed long ago, may furnish some particulars illustrative of the explanation he has given. by long continued heat.

In 1782, Mr. Ciffle, a porcelain manufacture at Lunéville, sent me several specimens of glass of different qualities, rendered opake by the long-continued action of heat, without having been surrounded by a mixture of sand and gypsum in Reaumur's mode. The five pieces, which I lay before the class, made part of these specimens, and have the original labels still fastened to them. Specimens, by Mr. Ciffle.

No. 1 is a piece of common window glass, 13 cent. by 10 [5 in. by 3·9†], which, by exposure to the strong heat of a porcelain furnace, without any cement, has become absolutely opake, and very white, without any alteration of its shape; and has acquired much greater hardness and solidity. No. 1.

No. 2, a piece of the same window glass, exposed in the same furnace, and touched by the flame, has also become opake, and of a fine white in the fracture; but the surface has a yellowish tinge. No. 2.

No. 3 is a piece of bottle glass, kept in the fire in charcoal dust, equally become opake, of a fine white internally, with a uniform shining coat of brown black over all its surface. No. 3.

No. 4 is a piece of a bottle, which has undergone the heat of a porcelain furnace surrounded with powdered soot. It has acquired a coat of a deep bistre-colour, but within is completely devitrified, and equally white. No. 4.

No. 5 is the bottom of a bottle, which was exposed to the most violent heat without being surrounded with any thing, and has become white and opake throughout its whole thickness. No. 5.

At the time of these experiments by Mr. Ciffle, and indeed some years before, Mr. James Keir had announced, that glass might be rendered opake by long continued an- Glass devitrified without cement by Mr. Keir.

* *Mem.* de l'Acad. des Sciences, 1739.

† It was longer when it came to my hands, but I reduced it to these dimensions, in order to make experiments with some pieces of it.

nealing,

nealing, without any cement; and that in this state it was more dense, and less liable to break by a sudden transition from cold to hot, or the contrary. The latter property was confirmed by the experiments of Mr. Ciffle, so that he did not hesitate to consider glass so altered as the most proper substance for supplying chemistry with vessels at once refractory and not liable to crack.

The effect ascribed to crystallization

Mr. Keir, after having described these phenomena, ascribed it to the crystallization of the vitreous matter; an opinion naturally arising from the aspect of the fracture, which, instead of being conchoidal, as in transparent glass, exhibits, if not facets, at least very decided parallel striæ.

This supported by the experiments of d'Artigues.

The observations of Mr. d'Artigues strongly support this explanation. I have myself a mass of glass, found five years ago at the bottom of a crucible at the manufactory of St. Gobin, which appears formed to afford a demonstration of it; since we can distinguish, even with the naked eye, prisms shooting from the devitrified crust that constitutes its surface, and which is 2 or 3 mill. [about a line] thick.

But is this always the case? and is there a precipitation? Experiments by d'Arcet.

Is it true however, that all these changes are solely the effect of a crystallization? and can we admit with Mr. d'Artigues, "that a precipitation takes place throughout the mass, each of the component parts obeying at the same time the laws of attraction"? Before I attempt to solve these questions, I shall add a few more facts, resulting from experiments on this subject made by Mr. d'Arcet, and the consequences of which will naturally find a place in this discussion.

Specimen 1. Bottle glass completely devitrified.

Among the ten specimens from these experiments, which he put into my hands, No. 1 is a piece of bottle glass, which was exposed for three days to a heat of 50° of Wedgwood in Reaumur's cement. The devitrification is complete; interiorly it has a rosy tinge; the fracture exhibits striæ, arranged in stars, to the very centre; it gives no signs of electricity by friction; it rather scratches rock crystal, than is scratched by it; corundum leaves a mark on it scarcely perceptible through a lens.

Specimen 2. Another with lead.

No. 2, exposed to the same fire, the same time, in the same cement, has barely acquired an earthy crust, which is scratched

scratched by rock crystal. The interior has remained of the nature of a green glass, transparent, and forming a geode by the contraction of the matter, while adhering to the crust. This glass contained oxide of lead.

A geode.

No. 3 includes two artificial intaglios. They are made of bottle glass, moulded first in a cupelling furnace on an impression taken with tripoli, and then devitrified at a heat of 81° of Wedgwood. They are not electric by friction even on the polished faces. Their specific gravity is 2·801. Corundum scarcely leaves a visible trace on them. This hardness, which is such that it allows them to be used as dies, and the chastity with which impressions from them represent antiques, have introduced the products of these trials among the ornamental arts; and this not only for intaglios, but also for cameos, the figures of which are now executed of a different colour from the ground, by the addition of coats of glass of a different composition, to imitate onyxes; devitrification being afterward employed, to give them that hardness, which is the principal characteristic of precious stones. I shall say no more of this new art, the processes of which, we may easily foresee, will be improved by that industry, which will also extend its applications.

Specimen 3. Artificial intaglios of bottle glass.

Cameos.

No. 4 is a piece of a globe of the same glass, cut to be used as a capsule, and afterward devitrified in Reaumur's cement at 50° Wedg. The fragments with it, being from a similar capsule, show the striated fracture. Corundum scarcely leaves a visible mark on them, and they are not sensibly electric by friction. These fragments may be heated red hot in the fire, and immediately thrown into water, without losing any thing of their solidity. I have kept them in sulphuric acid in the strongest fire, and they have come out without the least alteration, or loss of weight.

Specimen 4. A capsule.

No. 5 is remarkable for the differences it exhibits. It has still the vitreous fracture; is evidently translucid at the edges; becomes electrical by friction; and is scratched by silex. Accordingly it differs from bottle glass only by the appearance of a grayish white porcelain or enamel, which it has acquired in losing its transparency. But these differences may be accounted for by the process employed in this

Specimen 5. Effect of slow refrigeration merely.

[redacted]

this experiment, the object of which was, to learn what would be the result of slow refrigeration alone. It is evident, that the heat was not carried to a sufficient height, or that it was not continued long enough, to complete the devitrification.

Specimens 6, 7, and 9. Stained glass.

Red.

No. 6, 7, and 9, are the results of trials to devitrify pieces of stained glass from church-windows, coloured red by oxide of gold, and blue by oxide of cobalt. The first two, in losing their transparency, have acquired a purple tinge: but one of them, which had lead in its composition, had but little consistency, and was interiorly full of blebs, and as it were spongy; while in the other the devitrification had pursued its usual and regular course from the two surfaces, leaving in the middle a remainder of vitreous matter, which would have disappeared on a longer continuance of the fire. This exhibits some signs of electricity by friction; and both are scratched by rock crystal.

Blue.

The fragment stained by cobalt announced by the aspect of its fracture, which was still a little vitreous, that the devitrification was not far advanced, It had lost all its transparency however: and its blue colour, though weakened in the mass, was much more intense at the surface. It was still a weak insulator. Its hardness was such, that corundum scarcely made a perceptible impression on it.

Specimen 8. Farther proof, that devitrification begins at the surface.

No. 8 is remarkable as a fresh proof, that devitrification always commences at the surfaces, and proceeds gradually to the centre, when the heat is continued long enough. This piece resembles a small geode, the crust of which, completely devitrified, includes a portion of the substance still in the state of perfect glass. We shall find, that these accidents equally occur in the devitrifications by the fire of volcanoes.

Specimen 10. Attempt to form an intaglio, after the glass was devitrified.

No. 10 exhibits a result still more interesting. It is an attempt to form an intaglio, not by moulding it in the state of glass, and devitrifying it afterward; but by devitrifying it previous to its being placed on the mould, to receive the impression. The fusion has produced a very homogeneous mass, of a dull gray colour, which exhibited, though imperfectly, an impression of the figure in relief on which it had been cast; while its completely vitreous fracture,

ture, and translucency on the edges, evidently announced a return to the state of glass, as far as it could in the proportions of its present composition.

From these characters I could not avoid suspecting, that there must be a correspondent change in the specific gravity. This was fully proved on trial; for that of the mass thus brought back to the vitreous state was only 2·625; while that of the same glass, when completely devitrified, was always from 2·77 to 2·801.

Mr. d'Artigues has rightly observed, that glass, when devitrified, is not so bad a conductor of heat and electricity as before. In fact we have seen, that several pieces of different kinds of glass, when brought to this state, no longer exhibited any sign of electricity by friction. If it were possible to doubt, that this property depended more on the nature of the component parts, than on the manner of their arrangement, we should be obliged to return to this principle from the result of the experiment, in which devitrified glass, restored to its former state by being fused again without addition, and having resumed its original density, fracture, and other characters (except the transparency, which appeared only on the edges), showed no more disposition to become electric by friction than before.

Electricity dependent on the nature of the principles of bodies, not on their arrangement.

All the products of devitrification I have hitherto mentioned concur in showing, that it commences constantly at the surfaces: and this is a fact of sufficient importance for us to inquire into the real cause of such accidents, as may give rise to objections against this principle.

Devitrification always commences at the surface.

Are there in reality instances of a devitrification effected interiorly; or between two portions of unaltered glass? A plate from the glass-house of Prémontré, given me by Mr. d'Arcet, appears, at first sight, to show the possibility of this. The part completely devitrified forms a very white stratum, absolutely opake, 5 or 6 mil. [2 or 2·4 lines] thick, between two strata, rather thicker, of green glass, which have retained all their transparency, and exhibit the vitreous fracture very decidedly contrasted with the striated fracture of the devitrified part.

A specimen seemingly contradicting this:

But, on a careful examination of this piece, we soon perceive, that it was not cooled at rest; and that a portion of the

but in fact a proof of it.

the glass, which was still fluid beneath the superficial stratum, that had become opake and more refractory, was carried above it by the motion of the crucible, in taking it out of the furnace. A comparison of it with another plate of the same glass, in which we find only the two strata in their natural order, appears to me to leave no doubt of the truth of this explanation.

Obs. V. *Devitrification of glass by the fire of volcanoes.*

Supposition that volcanic differ from common fires.

The hypothesis framed by the celebrated Dolomieu is well known: that the fires of volcanoes do not act like those of our furnaces; that, though they produce prodigious effects, their activity is not great; that the fluidity they occasion is not that of vitrifying matter; and lastly, that even the most fusible substances, included in the body of rocks, might have flowed in burning torrents, without having undergone any perceptible alteration*.

Supposed proof of this in glass buried in lava at Torre del Greco.

He imagined he had found a proof of this in the state to which pieces of glass had been reduced at the time of the dreadful eruption, that covered Torre del Greco, in 1794. This glass, the shape of which was still distinguishable, had become of an opake white. The alteration extended sometimes throughout its whole thickness: sometimes it left glass still untouched, with its original colour and transparency, between two opake crusts. Dolomieu laid before the class several specimens of these glasses, found in digging on the spot. He was so good as to present me with a few specimens, some of which were authenticated by volcanic scoriæ still adhering to them†: and I promised him in return several fragments, found in a furnace [*four à étendre*], where, as is too frequently the case for the manu-

Similar effects in our glass-houses.

* Journ. de Phys. vol. XXXVII, p. 193. Journ. des Mines, No. 22, p. 55.

† Mr. Breislak mentions in his Tour through Campania, vol. I, p. 280, a piece of window glass bent in different directions, the surfaces of which were converted into Reaumur's porcelain, while the interior retained the state and appearance of glass. Dr. Thomson, in his catalogue of substances found in the lava of 1794, had already described fragments of glass thus modified, to which he gave the name of glass-stone.

facturer's

facturer's profit, the broken glasses are raised on the sides, to remain till the working ceases, or till their accumulation obliges it to be stopped for emptying the furnace; in which he would find the same alterations, and the same progress of devitrification.

Dolomieu, having seen them in my collection, with prof. Pfaff, of Kiel, who was then in Paris, frankly confessed, that he had nothing to object against the identity of the effects of the glass-house fire and the fire that had acted on the glasses found at Torre del Greco; and he selected a few specimens for his collection. which convinced Dolomieu.

The fact, which authorizes us to compare the effects of the fire of volcanoes and of our furnaces at equal intensities, is supported by experiments, communicated to me by Mr. d'Arcet, which are equally interesting for their practical application, and the consequences deducible from them respecting the formation of basaltes. Experiments in confirmation by d'Arcet.

It is well known, that basaltes fuses about 60° Wedg.: and, as sir James Hall has very justly remarked, the product of this fusion is a glass, having all the characters and properties of volcanic glass*. I have obtained some myself, in pretty considerable masses, from the basaltic prisms of the extinct volcano of Drevin, which, after the operation, could not be distinguished faom the glass produced by the fusion of hornblende rock, touchstone, or vitreous obsidian lava. Sir James Hall's experiments.

Mr. d'Arcet tried the processes of devitrification on volcanic glass itself. He subjected to it pieces of 15 or 16 cub. cent. [9 or 10 cub. in.], and of spec. grav. from 2·775 to 2·784; and observed, that they were completely devitrified in the fire of a cupelling furnace; but, if the heat were carried to 50° Wedg. only, part, that had been before devitrified, returned to the state of glass. Devitrification of volcanic glass.

I need not remark the conformity of these results with those, which sir James Hall obtained by the slow cooling of basaltes, which he had first converted into glass; and on which principally he founded his opinion, that basaltes had been originally in a state of vitreous fusion. The results agreed with Sir J. Hall's.

* Journ. de Phys. germinal, an 7, p. 317. [See Journal, 4to ed. vol. IV, pp. 8, 56.]

Artificial touchstones of the best quality.

The volcanic glass, thus brought back to the state of very compact and very fine-grained lithoid lava, induced Mr. d'Arcet to have some polished, to serve as touchstones; and the trials he made leave no doubt, that they would supply the place of the native stones of the best quality, which are becoming very scarce.

Examination of what constitutes the real difference between transparent and devitrified glass.

Will crystallization account for the facts?

Can the facts I have recorded be explained by simple crystallization? or, to express myself more clearly, can they be reconciled with the known effects of this transition of substances to the regular concrete state, and with the hypothesis of a simultaneous precipitation of some of their fixed elements? This remains for me to examine.

1st objection.

In the first place, we may observe, that, if there were a crystallization and precipitation at the same time, the opake mass resulting from these would no longer be crystallized glass, but the product of its decomposition.

2d objection.

In the next place, if there were in reality a separation of some of the ingredients of the glass, they should exhibit, in some parts at least, the appearance of the colours, the degree of hardness, and the other characters peculiar to them; of which we do not perceive the least indication.

3d objection.

Lastly, on this supposition, the state of combination having ceased, the particles abandoned should be immediately given up to the chemical action of their solvents: but it is certain, that nothing is taken up from devitrified glass even by the most potent acids, assisted by a boiling heat. It must be acknowledged therefore, that their union still subsits; and even that it is more intimate, since it is this that constitutes bodies the most homogeneous, most solid, hardest, and most capable of resisting fusion and solution.

Devitrified glass fusible again when powdered.

According to Mr. d'Artigues, devitrified glass becomes fusible again, when, by reducing it to powder, the matter which had been separated, and which serve reciprocally as fluxes to each other, are again brought into contact. I conceived it was a proper subject for experiment to decide, whether

whether this fusion would restore the glass to its former transparency, and other characteristic properties.

I took a piece of the glass No. 1 of Mr. Cifflé (p. 59), that is, window glass devitrified without any cement, remaining white, perfectly opake, and of extraordinary strength notwithstanding its thinness. After having reduced it to powder, I put 7 gr. [108 grs] into a platina crucible with a cover, and raised the fire to 160° Wedg. The result was a mass tolerably well fused, but white, inclining slightly to greenish, and having barely some appearance of being translucid on the edges; very smooth at the surface, but beneath it full of little cavities occasioned by the ebullition. There was a loss of weight of 59 mill. [0·9 of a gr.] or a little more than 8 thousandths. Experiment.

It became an interesting inquiry, to find what change a refusion would produce in plate glass, in which the mutual saturation of the silex and its fluxes is commonly more accurate; and particularly whether in this also there would be a diminution of weight. Into a platina crucible I put 62 gr. [957·6 grs] of pulverized St. Gobin plate glass, and kept it for 3½ hours at a heat of 48° Wedg. The result was a mass completely fused, the frizzled surface of which [*surface ratinée*], to use the term of the glass-men, indicated a slight commencement of devitrification*, which had a yellowish tinge, and somewhat greater hardness than the interior; alterations that Mr. d'Artigues had already observed in glass, which, being more simple in its composition, and more perfectly combined, resists the continued action of heat much more. The great number of blebs, that had formed in the lower part, did not allow me to determine with accuracy its specific gravity; but there was a loss of weight of 2 dec. [3 grs], or a little more than 3 thousandths, without any circumstance of the process giving room for the slightest suspicion, that it could have been oc- Experiment with plate glass.

* This surface, examined with a lens, exhibits an immense quantity of small fissures, forming by their intersections prisms with unequal sides. By causing the light to pass through the two opposite fractures on the sides, rudiments of crystallization are perceptible beneath the superior crust, which also indicate the first effects of devitrification.

 casioned

casioned by any thing but the loss of this quantity of matter*.

To these strong reasons for rejecting the hypothesis either of a simple modification of structure, or of a precipitation of a portion of the component parts, let us add the increase of *hardness*, and *diminution of bulk*.

Farther argument from the hardness,

Among the products of devitrification, which I have laid before the class, there are several, as I have remarked, that cannot be scratched by rock crystal; there are some, on which corundum scarcely leaves a mark visible by a lens; and Mr. Ciffle's No. 5 scratches rock crystal, as an aqua marina would.

and density.

The density, that glass acquires in this process, is still more striking; though, like the hardness, it is only the effect of a more powerful aggregation. All the pieces, the specific gravity of which before and after the process I had an opportunity of comparing, gave a difference of 16 or 18 thousandths in addition. Mr. d'Arcet had two cubes of bottle glass, of the manufactory of la Garre, cut, for the purpose of ascertaining their bulk, by the scale of Wedgwood's pyrometer, before and after devitrification. One advanced 17°, the other 11°; which gave for the first the proportion of 1000 to 909; for the second, 1000 to 952. The glass having been cut from the same piece, and consequently being of the same quality, the difference between the results can be ascribed only to a greater or less portion of the cement adhering to the surface of these cubes; which, however, was far from compensating for the diminution of the original bulk. This is farther proved by the colour, which the pieces devitrified in Reaumur's cement assume at their surface; a colour, that often penetrates them to some depth, and can arise only from particles of metallic oxides contained in the sulphate of lime employed.

Diminution of bulk in devitrified bottle glass.

* Mr. d'Arcet has sometimes found the weight of the cubes of glass, which he subjected to the process of devitrification, increased 5 thousandths of a gramme on 2 grammes [31 grs]; but he operated in a cement. The same must have been the case with the pieces No. 3 and 4 of Mr. Ciffle, which, as has been seen, came out with a coating. But this proves nothing against the two experiments related above, made without cement, and in platina crucibles.

I think

I think then I may conclude, that the characters and properties, by which transparent is distinguished from devitrified glass, are not solely the effect of crystallization, either of the same integrant particles, or of some of its elements, which would form a new compound, the others being separated by precipitation; but that there is really a change of proportions in the compound, by the volatilization of a certain part. It is not when the progress of chemical analysis daily teaches us, that less than a thousandth part of its substance added, or substracted from a compound, produces striking changes in its properties, that we can admit the explanation of so many characters, and of such striking properties, simply by the mode of structure.

In devitrification there is a change of proportions by volatilization of some part.

XI.

Analysis of Olefiant Gas: by Mr. THEODORE DE SAUSSURE*.

THE inflammable gasses produced by the decomposition of vegetable substances were long considered as simple compounds of hidrogen and carbon: but, when the proportions of these elements were endeavoured to be ascertained by the quantity of carbonic acid gas produced in their combustion, and that of oxigen gas employed to burn them; it was found, that more water was formed, than ought to have resulted from the oxigen used: whence it was necessarily admitted, that the inflammable gas must have furnished the oxigen for this surplus of water. Mr. Berthollet has made the greatest number of experiments on this subject: he has subjected to analysis several inflammable gasses, obtained from the distillation of moistened charcoal, of oil, and of camphor; and he has found, that these gasses, all of which had been considered as compounds of hidrogen and carbon, and had been termed carburetted hidrogens, contain oxigen also, and should be

Carburetted hidrogen gasses presumed to contain oxigen.

Berthollet's experiments.

* Ann. de Chim. vol. LXXVIII, p. 57. Read to the Society of Physics and Natural History at Geneva, April, 1810.

called

called oxicarburetted hidrogens. The gasses, which I obtained by the decomposition of alcohol, and of sulphuric ether, in a redhot tube, were found, after accurate analyses, to be included in the same class *; and it was the same with respect to the hidrogen gas, that Dr. Thomson obtained from the distillation of peat †.

But olefiant gas not thoroughly examined.

After so many experiments concurring to prove, that oxigen is an essential intermedium in the aeriform combination of hidrogen and carbon, the examination of a farther number of compound inflammable gasses would seem superfluous; and it is no doubt for this reason, that the olefiant gas, obtained by subjecting to a gentle heat a mixture of alcohol with four times its weight of sulphuric acid, has not yet been accurately analysed. This gas, discovered by the Dutch chemists, is particularly distinguished from every other inflammable gas, as is well known, by forming an oil, when mixed with oximuriatic acid gas, and by furnishing more light, and more carbonic acid, when burned.

Obstacle to its analysis.

When olefiant gas is detonated with the proper proportion of oxigen gas for burning it completely, it breaks the strongest eudiometers. This has prevented Mr. Berthollet from adding its analysis to those I have mentioned above; and he made no attempt to surmount this difficulty, because it might be presumed, that the olefiant gas contained oxigen, from an experiment of the Dutch chemists; who announced, that this inflammable gas, if passed through a redhot porcelain tube, expanded, and acquired all the properties of the oxicarburetted hidrogen obtained from sulphuric ether by the same process‡.

Presumed to contain oxigen.

* See Journal, vol. XXI, p. 326.

† Ib. vol. XVI, p. 241.

‡ When the Dutch chemists mentioned these facts, they were unacquainted with the methods since invented for analysing inflammable gasses with accuracy; and consequently could not make this comparison with precision. It is probable, from other experiments, that carbon must have been deposited in the tube. This process requires many precautions, for us to place any confidence in its results. It is necessary, that the olefiant gas should be perfectly dry, and have no oxigen from the atmosphere mixed with it.

Dr.

Dr. Henry has made known the gaseous products of the slow combustion of the olefiant gas in an apparatus of his invention, which exposes the operator to no danger from the breaking of the vesels*; but from his results he has not deduced the consequences, that might be drawn, respecting the analysis of this gas. I shall exhibit them here, following the data of the English chemist. Dr. Henry.

* * * * * * * * * * * * * †

I proceed to the results, which I have obtained in repeating all these experiments. The author's experiments.

The olefiant gas was prepared by mixing pure alcohol with sulphuric acid in the proportions mentioned above. I interrupted the distillation before the white vapours, produced by the presence of sulphurous acid, were abundant. This sulphurous acid, which was in part in the state of gas, was absorbed by liquid ammonia. Mode of preparing the gas.

When I weighed the olefiant gas, the barometer was at 0·71893 [28·28 in.]. The thermometer attached to the barometer was at 3·75° [38·75° F.], and the temperature of the gas was the same.

The capacity of the receiver was 3527·8 cubic cent. [214·83 cub. in.].

The mercurial gage, in the receiver exhausted of air, stood at 3·5 mil. [1·38 line]. The difference of weight between the empty receiver, and the receiver filled with the olefiant gas at the extreme of moisture was 4·147 gr. [64·068 grs] without any correction.

The difference of weight of the receiver when empty and when full of atmospheric air, under the same circumstances, was 4·21 gr. [65·026 grs].

* Bibl. Brit. vol. XLI, p. 324. [See Phil. Trans. for 1808, p. 282; or Journal, vol. XXII, p. 83, for the original.]

† On comparing what follows here with Dr. Henry's Paper, it appears, that the French translator has made several mistakes with regard to the figures, by which Mr. de Saussure has been misled. What he says therefore does not apply; and, as of course it would be useless, it is omitted. C.

Hence

The specific gravity.

Hence it follows, that the weight of dry atmospheric air is to that of dry olefiant gas as 1000 to 985·2.

I have found by direct experiments at the temperature of 12·5° [54° F.], that the litre [2·1 wine pints] of dry atmospheric air at 0·76 of a met. [29·8 in.] weighs 1·228 gr. [18·967 grs]; consequently, from the ratio I have given above, the litre of dry olefiant gas weighs 1·2098 gr. [18·686 grs] at the same pressure and temperature.

Analysis in the eudiometer.

I now proceed to the analysis of the gas by its combustion over mercury in Volta's eudiometer. I have already said, that this instrument bursts, when olefiant gas is detonated in it, with nearly the proportion of oxigen gas requisite to burn it: but I prevented this accident, by employing a much larger proportion of oxigen than the olefiant gas could consume.

Mode of executing it safely.

Result.

I mixed 100 parts of the latter with 500 parts of oxigen gas deprived of carbonic acid by potash. These 500 parts of oxigen contained 23·5 of nitrogen, and 476·5 of pure oxigen. The mixture was reduced by detonation to 409·5 parts. Potash and hidrosulphuret of potash demonstrated in this residuum 201 parts of carbonic acid gas, 184·5 of oxigen gas, and 24 of nitrogen.

The combustion nearly complete.

After the separation of the carbonic acid in the residuum of the detonation of the olefiant gas, I examined whether the whole of the olefiant gas were burned, by adding to the residuum a small portion of hidrogen gas, measured with great precision, and detonating the mixture. By this second detonation not more than one hundredth of carbonic acid at the most was formed: the condensation of the gasses by the combustion was equal within a hundredth to what should have resulted from the action of the hidrogen gas I had added. The first detonation therefore had effected the combustion of the olefiant gas. In the calculations from this analysis I have paid no regard to the products of the last operation, because they are scarely to be distinguished from errours of observation.

Component parts of the gas.

From these experiments it follows, that 100 parts of this olefiant gas consumed for their combustion nearly 292 parts of oxigen gas to form water and 201 parts of carbonic acid. By comparing these numbers with the litre, and substituting the

the corresponding weight, we find, that 100 parts by weight of dry olefiant gas contain

Carbon	84·78
Hidrogen	13·55
	98·33

The sum of these products represents very nearly the weight of the olefiant gas, that I had subjected to analysis: the difference of a hundredth and half may be ascribed to errour of observation, or indeed to the small quantity of inflammable gas, that escaped combustion. Hence it follows, that the olefiant gas does not contain any observable quantity of oxigen, that it is composed of hidrogen and carbon, and that it should be termed *carburetted hidrogen*. It contains no oxigen.

Olefiant gas appeared to me to vary a little in its weight and composition, according to the mode in which it was prepared. When the distillation of the alcohol and sulphuric acid is carried too far, the gas obtained after the separation of the sulphurous acid is a trifle lighter, and contains a little oxigen. In my experiments, however, this oxigen never exceeded the four-hundredth part of the gas. Varies a little according to the mode of preparation.

In the processes which I conducted so, that there was no oxigen in the olefiant gas, I did not always find it precisely of the same gravity, or with the same proportion of hidrogen and carbon; but the difference amounted only to two or three hundredths, and consequently was not altogether independent of errours of observation. The difference trifling, when not contaminated with oxigen. In the experiment, in which I obtained olefiant gas of the greatest specific gravity and most loaded with carbon, its weight was precisely that of atmospheric air. Heaviest equal to atmospheric air. The litre [2·1 pints] of this dry gas weighed 1·228 gr, [18·967grs], at 0·76 [29·8 in.] pressure, and 12·5° [54·5° F.] temperature, 100 parts of this gas, mixed with 500 of oxigen, were reduced by detonation to 402; which contained 208 of acid gas, and 194 of oxigen; not mentioning the 12 parts of nitrogen mixed with the oxigen, which were found, to about a hundredth, in the residuum of the process. 100 parts of olefiant gas by weight therefore consumed 306 parts of oxigen gas, and formed

formed 208 of carbonic acid. Hence it follows, that 100 parts of olefiant gas by weight contain

Its component parts.

Carbon	86·43
Hidrogen	14·34
	100·77.

General conclusions.

From all these analyses I conclude, that olefiant gas, when properly prepared, contains no sensible quantity of oxigen. In this state its specific gravity is equal or but little inferior to that of atmospheric air. One part of this gas by bulk consumes nearly three of oxigen for its combustion, and forms two parts of carbonic acid.

Omitting fractions, olefiant gas contains, by weight,

Carbon	86
Hidrogen	14
	100.

Condensation of the hidrogen in it.

Fifteen parts of hidrogen appear to suffer a condensation to about half their bulk in dissolving 85 parts of carbon; and the olefiant gas thence resulting has by calculation very nearly the specific gravity, that I found in my first experiment.

XII.

Abstract of a Paper on the mutual Action of metallic Oxides and alkaline Hydrosulphurets: by Mr. GAY-LUSSAC*.

Mutual action of metallic oxides, and alkaline hydrosulphurets.

THE paper, to a sketch of which I here confine myself, includes the experiments I have made on the mutual action of metallic oxides and alkaline hydrosulphurets. I found,

1st, That the metallic oxides, in which oxigen is greatly condensed, as those of zinc and iron, do not decompose the hydrosulphurets.

2dly, That all the other oxides decompose the hydrosulphurets, and yield products, some of which vary according to the particular nature of the oxides.

* Ann de Chim. vol. LXXVIII, p. 86. The principal results were mentioned in a chemical lecture at the Polytechnic School on the 10th of April, 1811.

2dly,

3dly, That sulphuric acid is never formed.

4thly, That there are constantly formed water, and sulphites or sulphuretted sulphites; and frequently metallic sulphurets.

5thly, That it is consequently impossible, to obtain the bases of the hydrosulphurets pure by means of metallic oxides.

6thly, That, when a sulphuret is dissolved in water, there is never any sulphate formed, as was generally supposed, but sulphites, or sulphuretted sulphites.

Experiments

I shall relate some of the experiments, that led me to these results; and take first as an example the black oxide of manganese, and very pure and colourless hydrosulphuret of potash.

with oxide of manganese and hydrosulphuret of potash.

As soon as these two substances are mixed, their mutual action announces itself by a very sensible elevation of temperature; the hydrosulphuret takes an orange yellow colour, like the sulphuretted hydrosulphurets; and, when muriatic acid is poured in, sulphur is precipitated, and sulphuretted hidrogen evolved. On heating the mixture, it speedily loses its colour, and becomes as limpid as water. At this point the liquid, which is strongly alkaline, precipitates acetate of lead of a white colour; and it might be supposed, that it contained only potash: but, if muriatic acid be poured in, it immediately becomes turbid, sulphur is thrown down, and sulphurous acid gas is evolved. If, after having boiled and filtered, muriate of barytes be added, no precipitation takes place. Lastly weak sulphuric acid, poured on well washed oxide of manganese, dissolves cold a large quantity, without the evolution of any gas, particularly of sulphuretted hidrogen.

Hence it follows,

Results.

1st, That the first effect of the oxide on the hydrosulphuret is to convert it to the state of a sulphuretted hydrosulphuret, acting in this as the air does on hydrosulphurets, and very probably giving rise to a sulphuretted sulphite from the commencement of the process.

2dly, That a great deal of sulphuretted sulphite is afterward formed.

3dly, That no sulphuric acid is produced.

4thly,

4thly, That the black oxide of manganese is brought back to a minimum, and that no sulphuret of manganese is formed.

Oxide of copper and sulphuretted hydrosulphuret of barytes.

As a second example I shall take the brown oxide of copper, and the sulphuretted hydrosulphuret of barytes. These two substances act strongly on each other; and, if they be heated, the liquor presently loses its colour, and no longer contains any thing but barytes mixed with more or less sulphuretted sulphite. The oxide, after having been washed till the water that comes off is no longer precipitated by sulphuric acid, effervesces with muriatic acid in consequence of the sulphurous acid evolved, and a great deal of muriate of barytes is formed. The residuum, washed anew to remove the latter salt, and then treated with very weak nitromuriatic acid, leaves no other residuum than sulphur, which collects on the surface of the liquid.

The two experiments compared.

Hence we see, that the oxide of manganese and the oxide of copper, though exhibiting the same general result, have acted differently in this, that no sulphuret of manganese was formed, though sulphuret of copper was produced: but the cause of this is, the oxide of manganese was only reduced to a minimum, and in this state it has little affinity to sulphur.

Solution of sulphurets in water.

I shall not relate any more experiments of this kind, but shall conclude with a brief account of what happens, when a sulphuret is dissolved in water.

Sulphuret of barytes.

I made some sulphuret of barytes and sulphuret of potash with a gentle heat. The first, dissolved in water, left a residuum, which, after having been washed, dissolved completely in muriatic acid, evolving a great deal of sulphurous acid.

Sulphuret of potash.

The solution of sulphuret of potash, into which I poured muriate of barytes, yielded but a slight precipitate, which dissolved completely in muriatic acid. The mixture had been heated, and on cooling a great many little crystals of sulphuretted sulphite of barytes were deposited on the sides of the vessel.

Sulphuretted sulphites.

I found also, that the sulphuretted sulphites were not altered by exposure to the air; and that a neutral sulphite can dissolve a great deal of sulphur, without becoming acid or alkaline.

XIII.

XIII.

On the Ore of Platina of St. Domingo: by Mr. GUYTON-MORVEAU*.

IT had long been supposed, that platina was found only in the gold mines of Santa-Fe and of Choco in Peru. Twenty years ago there was a report, that some had been obtained from a ferruginous sand in St. Domingo; but apparently the examination of this was not executed so as to give decisive results, since it has not been published. The report, no doubt premature, of its existence in Siberia, has likewise died away. The singularity of such a limited and apparently exclusive situation remained attached to platina, till Mr. Vauquelin found as far as 10 per cent of it in the gray silver ore of Guadalcanal; where he has no doubt it is in the metallic state, but without being accompanied with either of the four metals lately discovered in the platina ore of Peru†.

Platina reported to have been found in St Domingo some years ago.

What Mr. Percy submitted to the inspection of the Institute, on the 12th of Feb. 1810, leaves no doubt of the existence of this metal in the eastern part of St. Domingo. It was brought thence by surgeon-major Dubizy, an enlightened naturalist. It exhibits precisely the same characters as that we have from Spain. The grains, equally flattened, are in general a little larger, and its specific gravity is a little greater; which may arise from its having been more carefully freed from foreign matter, though the magnet still separates some from it. It is found, principally after heavy rains, in the sands of the river Yaqui, at the foot of the mountains of Sibao. These sands, which contain also a little gold, are collected by women, who, without washing them, sell them for a few maravedis.

This lately verified.

Mr. Jeannety, who has begun to manufacture a few hectogrammes of this ore, informs me, that, having slightly calcined it, and afterward poured on it sulphuric acid, he perceived a few grains of gold among it.

Some gold with it.

* Ann. de Chim. vol. LXXIII, p. 334.

† Ann. de Chim. vol. LX, p. 317: or Journal, vol. XVII, p. 198. [Mr. G. M. should also have mentioned the ore from Brazil, of which an account is given by Dr. Wollaston, in the Phil. Trans. for 1809, p. 189. See Journal, vol. XXV, p. 28. C.]

SCIENTIFIC

Wernerian Society.

Two species of arctic gull.

AT the first winter meeting of this society, an interesting communication from Dr. Arthur Edmondstone was read, concerning the larus parasiticus, or arctic gull. Owing to the remote situation of the haunts of this gull, its history and manners have hitherto been little known. Dr. Edmonstone has now illustrated them. He has observed two kinds of arctic gulls in the Shetland Islands; the common sort, with the breast and belly of a mouse colour; and another sort, with the breast and belly pure white. Each kind keeps together; and the white is a larger and heavier bird, but less bold than the other. The doctor is therefore inclined to consider these not merely as varieties, but as distinct species.

Varieties of zircon in Scotland, and subspecies of rutilite.

At the same meeting professor Jameson read to the society a short description of several varieties of the precious stone named zirçon, which he had lately discovered, imbedded in sienite, in Galloway. He also informed the society, that he had observed, in the same rock in Galloway, both the brown and the yellow subspecies of that very rare ore known to mineralogists by the name of rutilite, or sphene.

Mr. Edgeworth's new invented spire.

Mr. Edgeworth informs me, that an iron skeleton of a spire, agreeably to his construction, described, vol. XXX, p. 241, may be covered with thin *flags* of Portland stone, or with any other thin flags or stones, that do not imbibe water, and that are of a pleasing colour: that the Board of First Fruits in Ireland, which consists of all the archbishops and bishops, thought proper, without any solicitation, to present the parish of Edgeworthstown with the cost of the spire: and that the Dublin Society have ordered a working model to be made of the spire, and the machinery employed in raising it, to facilitate the erection of such ornamental buildings in different parts of this country.

The views of the coast of Ireland from the Bay of Dublin are uncommonly beautiful; but the city appears flat and uninteresting from its having scarely any elevated building, the only spire in the whole city being that of St. Patrick's church. As the cause of this defect has probably been the expense attending the construction of steeples in the usual mode, we may now hope, that it will not long continue.

The

The annual publication called the *Ladies Diary* or *Woman's Almanack*, has every year, for upwards of a century, contained a certain number of mathematical problems, to be answered in the Diary of the following year. The publication of these has answered several valuable purposes; in particular it has awakened the attention of many to the study of the mathematical sciences, who would not otherwise have thought of them: the questions have served to exercise the ingenuity, and call forth the exertions of young mathematicians, some of whom have in time arrived at great eminence, as cultivators of mathematical learning: and, lastly, the work has served as a repository for the preservation of many curious mathematical disquisitions, which, but for this mode of publication, would never have been known to the world.

Information requested respecting the writers of the mathematical questions and their answers in the Ladies Diary.

The beneficial influence, which the Lady's Diary has exerted upon the state of mathematical science in this country, has been long felt and acknowledged; and has been particularly noticed by the writer of that very valuable analysis of the Mécanique céleste, given in the Edinburgh Review. Speaking of the comparative state of mathematical knowledge in England and on the Continent, he says: "A certain degree of mathematical science, and indeed no inconsiderable degree, is perhaps more widely diffused in England than in any other country in the world. The *Ladies Diary*, with several other periodical and popular publications, are the best proofs of this assertion. In these many curious problems, not of the highest order indeed, but still having a considerable degree of difficulty, and far beyond the mere elements of science, are often to be met with. And the great number of ingenious men, who take a share in proposing and answering these questions, whom one has never heard of any where else, is not a little surprising. Nothing of the same kind we believe is to be found in any other country.—The geometrical part has always been conducted in a superior style; the problems proposed have tended to awaken curiosity, and the solutions to convey instruction in a much better manner, than is always to be found in more splendid publications." —(See Edin. Rev. vol. XI, p. 282.

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Information requested respecting the writers of the mathematical questions and their answers in the Ladies Diary.

A collection of all the mathematical questions, as well as other parts of the Diary, from its beginning to the year 1772, was published about that period, by its present ingenious and learned editor, Dr. C. Hutton, late of the Royal Academy, Woolwich. That work however being now out of print, and the stock of questions now considerably increased, Mr. T. Leybourn, editor of the Mathematical Repository, has issued proposals for publishing by subscription all the mathematical questions, and their answers, from the commencement of the Diary to the present time. Beside the valuable notes given in Dr. Hutton's edition, the present editor intends to give others, and in particular, he means to give, as far as he can, brief notices of any circumstances he may be able to learn respecting such authors of the answers to the questions as are dead, and even of such as are alive, when it can be done with propriety.

But as many of the authors have now been dead for a number of years, and have not been known beyond the particular circle of their friends, he is aware, that this part of the work can only be rendered tolerably complete by the assistance of such friends to his undertaking, as may be capable of giving the information here specified. He ventures therefore, through the medium of the Philosophical Journal, to solicit communications respecting the authors of the mathematical parts of the Diary. These may be addressed to him at the Royal Military College, Great Marlow, Bucks.

London Hospital.

Medical lectures.

Dr. Buxton's Spring Course of Lectures on the Practice of Medicine, will be commenced about the 20th of Jan. 1812.

Anatomical Theatre, Bristol.

Chirurgical lectures.

Mr. Thomas Shute will commence his spring course of Lectures on Anatomy, Physiology, and the Principles and Operations of Surgery, on the 17th of January, at eight in the morning.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

FEBRUARY, 1812.

ARTICLE I.

On the Mechanism of Flowers. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

I AM now to give the mechanical management of the flowers.

Mechanism of flowers.

That each natural species has a different mechanism, one peculiarly suited to its shape and structure, I am perfectly convinced, from the dissections I have made of such numbers, having tried every sort of flower, I could procure. I am indeed not fortunately situate in this respect; as I am not near many gardens where curious exotics can be had, so that I am obliged to be contented, in a great measure, with what the indigenous soil and the common garden will yield, which do not produce me that variety I could wish. Indeed I did not intend to pick out the most animated flowers, lest it should be said, that they were exceptions to a general rule; I have rather taken the first that occurred, as there is no flower without mechanism, or the beautiful arrangement of which is not such as to be well worth the con-

contemplation of a philosophic mind: and when it is considered what a quantity of plants there are, and that each species has its appropriate contrivance, what an idea does it give us of the Creator of all!

The mechanism of the polygalæ.

I shall begin with the mechanism of the polygalæ, all the species of which are, I believe, managed in the same way. To defend the stamen and pistil, till the seed is to be impregnated, a hood covers them; but as it is also necessary, that the sun should, at the same time, shine on the female, the hood has a gatherer behind, see Pl. III, fig. 1, *a a*, to raise or depress it. When the sun appears, the contraction of this gatherer, in which runs the spiral wire, draws back the hood as at fig. 2. *b b* is a small ball, on which it turns as on a swivel. When it falls back, the drops appear on the pistil, and are seen saturated with the powder of the stamen; but if a cloud should in the mean time obscure the sun, and the wind rise, the gatherer behind would directly be lengthened, the hood return to its place, and the drops of the pistil retire within the orifice.

How often have I watched this beautiful process! Seated on the ground I have at pleasure made the hood rise and fall by the help of the sun, and a wet napkin, alternately admitted over it; thus viewing all its mechanical wonders at once.

The flower dilates and contracts.

The wings, represented at fig. 2, 3, 4, have also a part which contracts and dilates, fig. 3, *d d*. The spiral wire, meeting at *e e*, fig. 4, is fastened to the exterior edge of the wing, passes thence to *c c*, where it forms knots, and is then collected and drawn together to a sort of pin, round which it winds, while the outside part, fig. 3, *d d*, gathers up like a gatherer, and throws the wing from it, or draws it near the body, alternately, as fine weather, or bad requires it; as the part *f f* will necessarily open it by the contraction of the spiral wire, and throw it from the body of the flower to admit the sun to the seeds, that they may be perfected by its rays.

On examining a flower this effect will be easily seen. The lengthening of the filaments also serves to keep the hood on, when the wind blows; but when the sun shines, they contract, and the hood is again at liberty to retire back

back. It seldom falls quite off. It is curious, that the species of swivel, which belongs to it, is common to plants in general; but is so contrived, that the part, within which it turns, is but three quarters of a circle, see fig. 5, so that if with the finger you turn the hood quite down, it comes off. The same piece of mechanism is foundin the phaseolus leaf, and in many flowers. It is inconceivable what a perfect weather glass this flower is: not a cloud can pass over the sun, that does not change its position; and every night the whole flower is drawn up close, and shelters itself under the leaves.

Mechanism of the phaseolus.

The next flower, which I shall consider, in some measure resembles the last, being a diadelphian, the phaseolus vulgaris. Here the principal contrivance is the contracting of each part to the form of the pistil, by the means of the spiral wire. The wings are moulded in the same model, so that by drawing the spiral wire tight, it must necessarily slip off the part on which it is placed; and by this means throw itself to a certain distance from the body of the flower. In its perfect state, fig. 6, *g g*, Pl. IV, is fastened on to *h h*, figs 7, 8: of course, when the top of *g g* is contracted it will push off *h h*, by which means the flower opens to the sun, and sends back the banner fig. 9; which contracting in its turn by the means of two plaits under the collar *i i*, it can wave itself backward and forward to admit the sun, or to shade the body of the flower from it. In the same manner the part *g g*, fig. 6, by contracting, fills it up, and, drawing the side which projects, almost turns the wings round, and throws the banner at a distance, leaving the pistil to twist till the keel *w*, figs 7 and 8, is broken, which exposes the female to the sun. Then the usual process takes place. I have observed, that, when the phaseolus grows in a hothouse, the keel generally breaks; but it appears, that the temperature is not high enough, to produce this effect in the open air in this country: we properly therefore can trust the indigenous flowers only, to teach us to understand the real motion of a flower. Nature certainly intended the keel to break, and expose the pistil to the sun's influence, while the drops appear on it: so the keel generally slips off from most of the diadelphian

Difference in a hothouse.

G 2 plants

plants at that time. This female is also another proof of the spiral wire being inserted without a case, and therefore twisting much.

Effect of the frost and lightning.

How powerful is the mechanism of the antirrhinum! The strength of the spring is such, that it appears as if made of steel, rather than of a thin cuticle, with a pabulum of diminutive round bubbles of water. There is not any thing more wonderful than the strength in flowers, and in herbaceous or annual plants. I could scarcely believe, that it could proceed from their fulness alone, and that when I found a stem of cabbage as hard as wood, a frost could in one moment render it as soft as pap: and yet it is certainly so. Lightning and frost produce exactly the same effect on them. On examination of the stem of vegetables, after being thus struck, the vessels appear broken, and the liquid discharging itself. It is the same with a leaf or flower: the strength of the corolla depends wholly on the balls of water, which forms its pabulum, and the vessels, which are partly wood, partly spiral wire. Press them lightly between the finger and thumb, the bubbles are broken, and the whole becomes a soft wet rag. There is however a curious part of this process I cannot at all comprehend; it is the circular turn both frost and lightning produce in all the thick stems of herbaceous or annual plants. When thus struck, their parts are found not only broken, but twisted, so that the whole interior will be entirely misplaced, and all one way. But I should apologise for this digression.

Mechanism of antirrhinum.

The spring of the antirrhinum is formed by the thick wood vessels *r s*, fig. 10, turning within that which surrounds them; so that when the spiral wire draws these tight, they push against the springs, and the flower opens its mouth. But there are many other motions caused by the pressure of the filaments, contracting and pushing crossways against the opposite sides of the flower; while the bending of the fibres *n n*, from their interior contraction, alternately presses each stamen to the glutinous drop of the pistil, now ready for their reception. There is not a more beautiful arrangement than this, which may be seen by pressing the two sides so as a little (when the sun shines fully

fully on it to open the flower; but I doubt not there is in this part of the process some degree of electricity, which contributes: as it is only in a particular state of the pollen, that the stamen bends towards the pistil; it now does so but when the sun shines; and if you shade the flower, you stop the effect, at least prevent the other stamens proceeding in turn. The mechanism that makes it move toward the pistil is plain and evident: but that it should move only when the pollen is perfectly ripe, I must believe owing to some electric attraction between the liquor of the pistil and the flower of the stamen. About that time, if the antirrhinum is watched, it is in perpetual motion in the interior; and indeed this is the case with most flowers: but if one of the stamens is abortive, this remains totally still though on examination it has exactly the same mechanical formation, by which the others are impelled toward the pistil*. I shall, when the season returns, give the tulip, the violet, and the bianca, but I shall wait till they appear in summer, in hopes to tempt young people of my own sex to follow me in the examination. There are many flowers, the mechanism of which is far more curious than that of those already described. But the contrivance evinced in the formation of the stamens and pistils far exceeds that in the corolla. The pistil of the violet has a regular trap to catch insects. The drop appears within a cup, when the juice is ripe; but if any fly attempts to place its proboscis within this cup to plunder it, a bag draws up, and closes the entrance in so quick a manner, that the poor insect within breaks off the part caught, or dies in consequence. But as it is only at the time of the impregnation of the seeds, that the drop appears, few are the victims of this mechanism. It is a great mistake to suppose, that there are only a few flowers so formed.—There is hardly any one, which has not some contrivance to protect the chief part of the honey, to secure the impregnation of the seed. Nature ordains, that the insect tribe should take that which remains generally at the

Perpetual motion in the interior.

Mechanism of the pistil.

* Is not this a sufficient proof, that, though mechanism is necessary to the motion of plants, the *cause* of this motion is something very different from mechanism? C.

bottom

bottom of the cup of the corolla; but if it even permitted them to steal also the drop it shows, which attracts the flower of the stamens, the seeds would seldom be impregnated.

Mimosa sensitiva.

I by no means think, that the spiral wire being, or not being, the cause of motion in plants, is decided by our completely understanding the management of the mimosa sensitiva. I should as soon think, that steam being the governing principle of the steam-engine depended on our perfectly comprehending the conduct of the machine. It is to the complete organization of plants in general, I trust, where their mechanism is found far more simple, and easily proved: not to a plant, the formation of which differs so entirely from every other, as to require the most excessive nicety in weighing the different powers against each other. That it is a mechanical object its formation alone proves. The rest, when I better understand this part of botany, I shall hope more plainly to show,

I am, Sir,
Your humble servant,
AGNES IBBETSON.

I shall add a short description of the parts of the flower:

Plate III, fig. 1—5, the common milkwort.

Fig. 2 shows the cap thrown back, to admit the sun's rays.

Fig. 3 the wing, separated from fig. 1, to which in its natural state it is fastened at *x x*.

Fig. 4, the same dissected, showing the manner in which the spiral wire is collected from the exterior of the wing, and conveyed thence to the body of the flower by means of the pieces *e e*, *f f*.

Fig. 5, the ball, or swivel, on which the hood turns, as shown at *b*, figs 1 and 2.

Plate IV, fig. 6, is the wing of the phaseolus vulgaris; fig. 9 the banner: fig. 8 the pistil, clothed with the keel; and fig. 7, the same without it. *w* is the part which breaks, to set at liberty the pistil in the interior. Though it will perform its functions without this, yet I should suppose not so well.

Figs 10, 11, the antirrhinum divided into two parts.

Fig.

Fig. 12, *rs*, shows the spring, by which the flower is opened and shut, as it is when twisted. By pressing *t r*, fig. 10, the flower is opened; *v v* are the nectaries, which increase the strength of the flower. I shall give the stamens and pistil, when I show the mechanism of the parts, which will prove how much the wood vessels *n n* contribute to the management of the flower by the interior drawing of the spiral wire.

II.

Remarks on Mr. ANDERSON'S *Experiments "On the Decomposition of Water in two or more separate Vessels", with an Account of Mr.* MURRAY'S *Experiments on the same Subject.*

To W. NICHOLSON, Esq.

SIR,

IN the number of your Journal for November I have read with much pleasure some experiments on the "decomposition of water, in two or more separate vessels," by Mr. Anderson of Perth. They were professedly made to correct an inference drawn from an experiment of Ritter, the result of which led some persons to suppose, that the elements of water could be transmitted, in opposite currents, through the substance of a metallic wire; while others, unwilling to admit the permeability of metallic matter by gaseous bodies, were disposed, from this experiment, to doubt altogether the commonly received opinion with respect to the compound nature of water. Inferences from the decomposition of water in separate vessels:

Mr. Anderson first repeated the experiment of Ritter, and found, that water was decomposed in the manner that had been stated; and employing afterward an apparatus, in which all the gaseous products could be collected, he likewise ascertained, that they consisted not of oxigen only in one receiver, and of hidrogen in the other, as Ritter had supposed; "but these two substances were found in each receiver, in the exact proportion in which they combine together to form water". Consequently, water had been decomposed Both oxigen and hidrogen found in each.

decomposed in each receiver, and no transmission of its elements, in opposite currents, through the connecting wire, can be supposed to have happened: neither is there any ground for considering water as a simple body, but a compound of the two elementary substances above stated.

The fallacy of Ritter's statement already shown by Mr. Murray.

By these experiments, then, Mr. Anderson has succeeded in detecting the fallacy in Ritter's experiment, and the consequent errours in the conclusion drawn from it; but, from not being acquainted with what has been done by others, he is not correct in the observation, that "no person "appears to have suspected the accuracy of Ritter's statement, or even to have repeated his experiments with any "degree of care". The truth is, this philosopher's experiment was repeated more than twelve months ago by Mr. Murray, and the fallacy of his statement completely detected.

His account of it.

In his Elements of Chemistry, published in October, 1810, Mr. Murray (vol. 1, p. 308) refers to this experiment of Ritter, in which, says he, "it is stated, that when "a wire attached to the positive side of a galvanic battery is "placed in water in a tube, and a wire from the negative side "is placed in another portion of water in another tube; and "when these are connected, not by placing them in a vessel "of water, but in separate vessels connected by a metallic "wire; the usual phenomena are produced, and the oxigen "is evolved at one wire, and the hidrogen at the other". Mr. Murray then goes on to state, that Ritter, conceiving it impossible, that the elements of water could be conveyed through the metallic wire, was led to conclude, that "the "communication merely of positive and negative electricity "to water caused it to assume these gaseous forms. Were "the fact as it is stated," continues Mr. Murray, "the "conclusion would perhaps follow. I have found, however, that it is a mere deception. The connecting wire "becomes a galvanic one, and its two extremities becoming electrical, by what electricians have denominated "*position* or *induction*, are in states of electricity the reverse of the galvanic wires in the tubes; and hence oxigen and hidrogen are evolved at their extremities, corresponding with the hidrogen and oxigen evolved at the "others;

"others: the extremity of the connecting wire, for example, "in the tube, in which the positive galvanic wire is inserted, being negative, and its other extremity, in the "tube in which the negative wire is inserted, being *positive*, "and therefore giving off oxigen corresponding to the "hidrogen, which appears at that wire".

These facts, ascertained by Mr. Murray, have likewise been mentioned in a late work of Mr. Ellis*, who gives the detail of another experiment, made by Mr. Murray, in which the phenomena were rendered very obvious and striking. "The wires of the battery were made to pass through "glass tubes, and the tubes were then placed in two "glasses, which were connected by the metallic arc. Instead of water, however, both the tubes and glasses were "filled with an infusion of red cabbage, which held a "neutral salt in solution. As soon as the electricity was "put in motion, the neutral salt, in each tube and glass, "was decomposed; and the effects were at once conspicuous "on the vegetable infusion. For on the side connected "with the *positive* end of the battery, the fluid in the "tube was reddened, while, in the glass of the same side "it was rendered green. On the contrary, the fluid in the "tube connected with the negative side was green, and in "the glass of the same side it was red. Hence decomposition had taken place on each side: and while the positive "pole of the battery attracted, as usual, the acid which "reddened the infusion in the tube of that side, the negative extremity of the arc attracted the alkali in the glass "below, and changed its fluid to a green: and by the opposite electricities of the respective wires, reverse effects "were produced in the fluids of the tube and glass connected with the negative side of the battery".

Mentioned also by Mr. Ellis.

Another experiment of Mr. Murray's.

Although it appears, that Mr. Murray had not only suspected, but actually detected, the fallacy in Ritter's experiment, long before the publication of Mr. Anderson's essay, yet I am far from insinuating, that the latter gentleman was at all acquainted with what the former had done. Indeed the train of thought, which seems to have suggested

Mr. Anderson's experiments confirm these.

* Farther Inquiries concerning Vegetation, &c., p. 181.

the

the experiments of Mr. Anderson, the methods which he followed, and the explanations which he has subjoined, all bear witness to the originality of his views; while the manner in which his experiments are executed and detailed is alike creditable to his ingenuity and skill. Still it is right, that the merit of *priority* should be bestowed where it is justly due; and it is this consideration alone, that has induced me to make the present communication. I will add, that I do not at all regret, in this instance, the circumstance of Mr. Anderson's being unacquainted with Mr. Murray's experiments, since it has prompted him to institute inquiries, which he has shown himself so well able to conduct, and the results of which so satisfactorily confirm the conclusion, at which Mr. Murray had arrived.

The theory of induction renders the supposition of any passage of the electric fluid unnecessary.

I shall conclude by observing, that the electrical law of *induction*, which Mr. Murray has pointed out, as affording an explanation of the *manner* in which these decompositions are effected, renders it unnecessary to resort to the supposition of a conveyance of electric matter, in opposite currents, through the water and the wire, in the way which Mr. Anderson has suggested.

I am, sir,

Your very obedient servant,

A. Z.

III.

Observations on some Phenomena of Electro-Chemical Decomposition: by GEORGE JOHN SINGER, *Lecturer on Chemistry, and Natural Philosophy.*

Subject of the paper.

THE subject of the present paper occupied a considerable portion of my attention about two years since. The results of my observations have been detailed in my public lectures, but I have delayed their publication, till the progress of my inquiry should admit a systematic exposition of its objects. The contents of a paper in the November number of this Journal, by Adam Anderson, Esq., induce me

me to alter this determination, and to publish (out of the order I had intended) my observations on the subject of his discussion.

Mr. Anderson's remarks on the decomposition of water in separate vessels.

Mr. Anderson has supposed a difficulty in the explanation of electro-chemical decomposition, when the products are collected in *separate receivers, connected by a metallic wire*. He has also stated, that Ritter affirms, when water is decomposed in such an apparatus, the oxigen and hidrogen must pass through the *connecting wire* in opposite direction.

This not more inexplicable than oxigen and hidrogen appearing separately at a great distance in one tube.

These statements are, I believe, by no means accurate; there has not been more difficulty experienced in the explanation of the experiment described, than exists in the most simple case of electrical decomposition, viz. The impossibility of conceiving, how the same particle of water is at once acted on by wires which are remote from each other. Oxigen and hidrogen are separately produced at the extremities of a tube furnished with gold wires, even when the length of the tube exceeds three feet, and any hypothesis, calculated to explain the phenomena in this experiment, will also explain them under any other modification of the apparatus.

Ritter's opinion favourable to electric energy.

The apparatus described by Mr. Anderson as Ritter's arrangement (Journal, vol. XXX, plate 6, fig. 3,) is not described in the paper to which he refers, (Journal, 4to series, vol. IV, p. 512,) nor is the opinion ascribed to Ritter advanced in that paper, or in any other of his very numerous and interesting writings, to which I have had access. On the contrary, from the tenour of his observations, published in the Bulletin des Sciences, Journal de Physique, &c. (translations of the most important of which have appeared in this Journal;) it may be presumed his opinion favoured the hypothesis of electric energy, recently so ably supported by Dr. Davy.

Mr. Murray's account of Ritter's experiments.

Mr. Murray in the last edition of his System of Chemistry, has mentioned Ritter's experiment, accompanied by some observations, which appear to consider the permeability of the connecting wire necessary to the explanation of the experiment. But this is not advanced by Mr. Murray as the opinion of Ritter; it is couched in the terms of a conclusion;

conclusion, which he has drawn himself. From these circumstances I am induced to believe, Mr. Anderson has taken his idea of this experiment from Mr. Murray's account, without examining the original paper; and has thus associated the arrangement and opinion of Mr. Murray with the name of Ritter. If I am wrong in this conjecture, I am indebted to Mr. Anderson for my errour; he has quoted the same paper and page of this Journal, to which Mr. Murray refers in his account; and, on a careful perusal of such paper, I find neither the experiment, nor the opinion described.

Ritter's experiments.

The experiments of Ritter, to which the above quotation refers, are detailed in a letter from a correspondent of Dr. Babington's; they are stated rather cursorily, as "some "account of the Galvanic labours in Germany." The lower part of an inverted siphon was filled with sulphuric acid, and its legs with water. When subjected to the action of the Voltaic apparatus, oxigen appeared in one leg, hidrogen in the other. This result I have constantly obtained, and it is the same in all cases when two *separate* vessels of water are connected by any *fluid conductor* not *metallic*.

Oxigen and hidrogen appear separate in vessels connected by a fluid not metallic.

Ritter from this experiment appears to have doubted the composition of water; he is said to have arranged an apparatus, in which two portions of water, in *separate* tubes connected by gold wire, evolved respectively oxigen without hidrogen, and hidrogen without oxigen. By employing *one tube* he is also said to have procured at pleasure from *the same portion of water* either oxigen gas *alone*, or hidrogen gas *alone*. This result I have never obtained; the arrangement I employed is precisely that described in the quarto Journal as Ritter's. Two gold wires are introduced in the opposite ends of a glass tube, sulphuric acid is poured into the tube till it rises above the point of the lower wire; the upper half of the tube is filled with water; when this tube is placed in the circuit, it is said, that wire only evolves gas which is surrounded by water, and that this gas is oxigen when the wire is connected with the positive end of the battery; and hidrogen, when its contact is made with the negative. In all my trials, both wires liberated

Oxigen or hidrogen said to be obtained at pleasure from the same water.

This result never found by the author,

but a mixture

ated gas, sulphur was frequently deposited, and the gas evolved was always a mixture of oxigen and hidrogen. Yet I have no reason to suppose my experiments defective, as the acid was concentrated, and remained distinctly separate from the water.

of the two gasses, and frequently sulphur.

The apparatus represented by Mr. Anderson (plate 6, fig. 3,) is frequently employed to procure the products of electrified water separate; and I have never observed an individual at all conversant with electro-chemical apparatus, surprised at the result. It is indeed impossible to examine the progress of an experiment of this kind, without perceiving the liberation of gas at the extremity of the wires connected with the battery, as well as at the extremities of that cemented into the receivers. *Both gasses* are *evidently* evolved from the water in *each* of the vessels A, B, C, D, but *one* only is collected; and it would be as irrational to suppose that the oxigen and hidrogen pass through the connecting wire in opposite directions, as it would to assert, that in the common experiment they pervade the voltaic battery, and the wires at its extremities.

In decomposing water in two separate vessels both gasses evolved from the water in each, but one only collected.

Mr. Anderson however states, that, when he first repeated this experiment, he thought it necessary to adopt the above opinion, or deny the composition of water. Suspecting afterwards there might be a positive and negative point in each vessel, he arranges an apparatus, and finds his conjecture verified; but these opposite states were at the extremities of the same metallic wire, and how is this effected?

This supposed contradictory to the composition of water.

Mr. Anderson has given an explanation, which supposes the positive electricity to pass from the zinc side of the battery through the water to the remote end of the connecting wire; and the negative electricity to proceed also from the copper extremity to the remote and opposite end of this wire. According to this supposition, *the positive must pass through the negative*, or the *negative through the positive*, without restoring an *equilibrium*; though they are acknowledged to have *a strong attraction for each other*, and to be respectively of *equal intensity*. Such an idea is at variance with every thing we know of electrical action; it is not supported by any of the analogies of the science; nor is it a legitimate

The opposite electricities supposed to pass through the same wire:

but this contrary to every received theory of electricity.

legitimate conclusion, according to the principles of *any* received theory. From the first experiments with the Voltaic battery, it has been an acknowledged fact, that the chemical changes, produced between two wires in any fluid, in one vessel, will be repeated in any number of such vessels connected together; provided the power of the battery is proportioned to the extent of the interrupted circuit. Such an arrangement was employed (soon after the invention of the battery) by Mr. Nicholson, and also by Mr. Cruickshank, who proposed the connexion of many tubes, as a means of producing a considerable quantity of gas. A similar disposition of the apparatus has been at different periods employed by most electricians; but the connexion of its phenomena with theory has been most clearly exemplied by the experiments of Mr. De Luc, in his analysis of the Voltaic pile*. This excellent philosopher has investigated with just attention the changes that occur when two tubes are employed; he has ascertained the electrical state of the different wires in the circuit, and finding the same *chemical effects* continue, when the wires underwent *changes* in their *electrical states*, he concludes, that the chemical effects do not depend on opposite electric energies, but on the passage of the electric fluid from metals to water and from water to metals; oxigen being evolved in the former case, and hidrogen in the latter.

Action of the Voltaic battery repeated at every interruption of the wires.

Mr. De Luc's experiments.

In the present state of our knowledge it cannot therefore be said, "that the important fact of a positive and a negative point at every interruption of the circuit is established beyond all doubt." The experiment, on which this assertion is founded, is but a repetition of Cruickshank's arrangement; and proves only, that chemical effects occur at every interruption of the metallic circuit. Until these chemical effects are shown to depend on the opposite electric state of the wires, or to be inseparable from such a condition, the existence of such opposite states must be considered as purely hypothetical.

A positive and negative point at each interruption of the circuit not proved.

The experiments of Mr. De Luc appear to me a sufficient indication, that the various phenomena of electro-chemical analysis are produced by the circulation of a single electric fluid;

The effects produced by the circulation of a single fluid.

* See Journal, vol. XXVI, pp 69, 113, 241.

fluid; the effects of which are modified by the nature of the bodies through which it passes. The experiments and observations I have made on this subject may be considered a sequel to this paper, and will form the subject of a future communication.

Prince's Street, Cavendish Square,
Nov. 15th, 1811.

IV.

An Attempt to explain the Phenomena of Caloric. In a Letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

IF the following attempt to explain the phenomena attendant on caloric will not disgrace your excellent Journal, I shall feel myself much honoured by its insertion.

It is a well-known fact, that caloric is the cause of the elasticity of gasses; and it is equally certain, that an electric spark, or the contact of an ignited body, will, in many cases, destroy their elasticity, and cause them to condense into a nonelastic substance. These facts may be exemplified by the decomposition and formation of water by electricity. If then, it may be asked, caloric be the cause of the elasticity of gasses; how can this elasticity be destroyed, by an addition of the same substance? Elasticity of gasses both caused and destroyed by caloric.

This apparent anomaly has been thus explained by Monge. As gasses are rarefied by heat, the spark will cause the sudden rarefaction of that part of the mixture, through which it passes: this will cause as sudden a condensation of the adjacent parts; the atoms of oxigen and hidrogen will thus be made to approach each other; they will therefore unite, and form water. Monge's explanation of this.

In a volume of Experiments on acetous acid, &c., published by Bryan Higgins, M. D., in 1786; I find the following, and I think more satisfactory, explanation of the phenomenon. Dr. Higgins's.

He supposes the particles of gasses, to be surrounded with distinct atmospheres of caloric; in which the densities are reciprocally as the distances from the particles, in a dupli- The particles of gasses not dissolved in caloric, but

cate

surrounded by atmospheres of it.

cate, or higher ratio. If the particles of the gasses were merely dissolved in caloric, there seems to be no reason, why they should not unite, according to their affinity for each other: for in a medium of uniform density, they would be impelled by equal, and contrary powers of the medium; there would therefore be no impediment to their motion, and consequently to their union. This being granted, the question still recurs: How does the addition of caloric facilitate their union? The answer now is obvious. By equalizing the density of the medium. It must be observed, that, if a mixture of oxigen and hidrogen be gradually heated, even to redness, in vessels which admit of their expansion; no union will take place*: for in this case, the atmospheres will preserve their relative density. But when the accumulation of caloric is sudden, as by an electric spark, the particles will not have sufficient time, to arrange it around themselves: by these means, the density of the medium, will be rendered uniform: the particles therefore, within the range of the spark, will unite; and, by their union, give out a quantity of caloric, sufficient to equalize the neighbouring atmospheres; and thus the whole of the gasses, if mixed in due proportion, will combine. Hence it appears, that the degree of ignition, which is necessary for the combustion of oxigen and hidrogen, varies directly as the density of the atmospheres; and inversely as the affinity of the particles for each other.

Oxigen and hidrogen heated red hot in contact without uniting.

This theory explains all the phenomena of caloric.

This is the explanation given by Dr. Higgins; and to me it appears much more satisfactory and intelligible than the common one. But the theory, I conceive, may be extended much farther; and will even be found to be sufficient to account for all the phenomena, attendant on caloric.

No two particles of matter can touch each other.

Let us suppose, that the particles of caloric are attracted by those of all other bodies with a force, which varies inversely as the distance from the centre of these particles: and also, that they are more strongly attracted by all other bodies, than the particles of these last are by each other; or, in other words, that the particles of all bodies, have a greater affinity for caloric, than for each other. This being

* This experiment requires repetition. I do not recollect to have met with it elsewhere.

granted,

granted, it follows, that every particle of matter will be surrounded with an atmosphere of caloric; the density of which will increase, as the distance from the centre of the particle decreases; and this probably in a duplicate ratio. Hence, no two particles of matter can touch each other*. This is a fact, which was proved long ago by Sir I. Newton.

When the attraction of the particles of a body for each other is so great, that the distance between them is less than the particles themselves, all motion among them must be prevented, and the body, consequently, will be solid. But, if the distance between the particles of a body be greater than the particles, yet, if the density of the atmospheres, which surround them, be very great, all motion may still be prevented; for it is evident, that they can only be moved by compressing the atmospheres; the density of which may be too great to admit of this compression by any force less than what would destroy the texture of the body: in this case, also, the body must be solid. It is probable, that the first case never occurs: the density of the atmospheres, therefore, is the cause of solidity. Hence the particles may be at a great distance from each other, and the specific gravity of the body vary little. The size, form, and weight of the particles, (all of which probably vary,) must also be taken into consideration. Constitution of solids.

Sir I. Newton supposed, that the particles of fluids were spherical; and in this manner he accounted for the facility with which they yield to the slightest impulse. But if the particles of solids be of different forms, (which may be considered as proved by Haüy,) it is difficult to conceive how they can be converted into spheres by the application of heat. But it is easy to conceive, that the accumulation of caloric will augment the atmospheres, and thus cause them to approach to the form of spheres, let the form of the particles be what it may. The same effect will Constitution of fluids.

* When I say, that no two particles of matter can touch each other, I speak of those which are homogeneal. It is possible, I had almost said probable, that the atoms of oxigen and hidrogen, in the state of water, do touch each other. This may perhaps help to distinguish chemical combination from cohesion, &c.

be produced, if the attraction of the particles for each other be diminished: for in this case the atmospheres will expand. Either of these causes, or both combined, will occasion the body to become fluid; for the density of the atmospheres will decrease as they extend: and thus the motion of the particles will be facilitated. The form of the particles also will influence the fluidity of the body. The more spherical these are, the less resistance will there be to their motion. Hence, to form a perfect fluid, the particles must be perfectly spherical, and placed at such a distance from each other, that the density of their atmospheres, where they come into contact, shall be 0: and this distance, probably, must be infinite. It must now be clear, that the specific gravity of fluids may be very various, and also that they may contain very variable quantities of caloric.

Constitution of gasses.

If we suppose the affinity of the particles of the body to be still farther diminished; or the sphericity of their atmospheres to be increased by a fresh addition of caloric; it will necessarily assume the state of a gas. A gas then is a more perfect fluid than a liquid is. Gasses also may contain variable quantities of caloric, and vary in specific gravity.

Bodies contain different quantities of caloric.

From all that has been said, it must be evident, that a solid may contain more caloric than either a liquid or a gas; and, of course, that the quantity contained in a liquid may exceed that in a gas. But the same body, when solid, will contain less caloric than when liquid; and less when liquid than in the gaseous state. It is also clear, that the specific gravity of a solid may be less than that of a liquid; and it is even *possible*, that the specific gravity of solids and liquids, may be less than that of some gasses.

We have seen the combustion of oxigen and hidrogen explained according to this theory; let us examine, how it accords with other chemical phenomena.

Composition of nitric acid.

When azote and oxigen are mixed in due proportions, and an electric shock is passed through the mixture; the atmospheres of those particles, through which the shock passes, are rendered of a uniform density, and the particles unite. But the combustion does not spread throughout the whole mixture; because the particles which combine

do

do not part with a sufficient quantity of caloric, to equalize the neighbouring atmospheres; the shock therefore must be repeated. Hence it follows, that nitric acid contains a large quantity of caloric: but, as its specific gravity greatly exceeds the mean of that of its component parts, the atmosphere, which surrounds its particles, must be very dense. It is needless to add, that this is confirmed by almost every experiment, in which nitric acid is employed.

Production of ammonia.

Again, azotic and hidrogen gas cannot be combined artificially, because the affinity, which these gasses have for caloric, greatly exceeds that which they have for each other. But when the hidrogen is nascent, its affinity for azote, being assisted by that of the surrounding bodies for caloric, causes their union. It may even be conceived, that the hidrogen seizes on a part of the caloric of the azote; and the whole quantity being divided between the two, the resistance is diminished; they then unite, and ammonia is produced.

Effects of compression on gasses;

The effects of compression on gasses may be thus explained. By bringing the particles nearer to each other, the density of their atmospheres is increased; they therefore exert a greater resistance, and the elasticity of the gas is increased also. If the compression be sudden and violent, a quantity of caloric is disengaged. It is exceedingly probable, that, in this case, the elasticity of the gas is injured. I have frequently thought, that I have observed this effect produced on atmospheric air; and I recollect to have seen the same remark in print, although I cannot precisely remember the place. If a mixture of oxigen and hidrogen be suddenly compressed, the heat, which is extricated, will equalize the atmospheres, and the gasses will combine. If the compression were gradually applied; it is probable, that no union would take place.

and of rarefaction.

It must be evident, that rarefaction will produce an effect contrary to that produced by compression. A part of the air being removed, the atmospheres will meet with less resistance; they will of course expand. By this means there will be occasioned, so to speak, a vacuum of caloric; it will therefore rush in from the neighbouring bodies, and thus cold will be produced; but, on readmitting the air, there will be a redundance of caloric;

 the

the thermometer therefore will rise. This also agrees with experiment. I have observed it several times this morning.

Effect of increased or diminished temperature.

The expansion and contraction of gasses, by an increase or diminution of temperature, will be easily explained. As the addition of caloric will increase the density of the atmospheres, expansion must ensue. The abstraction of caloric will occasion a diminution of their density; the attraction of the particles, therefore, for each other will prevail: they will then approach; and, in many cases, condense into a liquid, or even a solid substance.

Degrees of consistency.

There are many substances, as wax, tallow, &c., which assume various degrees of consistence in passing from the solid to the fluid state. This may be accounted for by the increasing sphericity of the atmospheres, which surround the particles, of which these bodies are composed: as this increases, their motion must be facilitated.

Constitution of metals.

The malleability, tenacity, and ductility of metals, are explained in a similar manner: the atmospheres not being too dense to admit of motion, when a considerable force is applied: and the attraction between the particles being sufficiently strong to prevent their separation. When the atmospheres are too dense to permit the motion of particles by the application of a considerable force, the body must be brittle, or exceedingly hard, according as the affinity of the particles for each other varies. When the atmospheres are very far extended, the body will also be brittle, or rather friable, from the diminished attraction of the particles.

Zinc.

When zinc is cold, it is brittle, because its atmospheres are too dense to admit of the motion of its particles; a small addition of caloric renders it malleable, by increasing their sphericity: and a still greater addition renders it friable, by separating the particles too far from one another. Perhaps the difference between hot short and cold short iron may depend, in some degree, upon a similar cause.

Hot short and cold short iron

Tempering of steel.

When ignited steel is suddenly plunged into cold water, it becomes brittle, owing to the separation of too large a quantity of caloric; but by exposure to heat, it may be made of different degrees of hardness, so as to answer various mechanical purposes. We may thus, I think, account for the effects produced by caloric in the tempering of steel: always recollecting,

collecting, that motion is facilitated by the addition of caloric. Elasticity is caused by the compression of the atmospheres, and their return to their former state.

Elasticity.

There is another phenomenon, which I believe no one has ever attempted to explain, I mean, the expansion of water by a diminution of temperature. We will suppose, with Mr. Dalton, that water is at its maximum of density at 36°. Its expansion by heat requires no explanation. Let us see how far its expansion by cold may be accounted for. It must be granted, that ice has a less affinity for caloric than water has, otherwise water would not give out caloric at the moment of its becoming solid. Hence it follows, that, when water is cooled, it must at last reach some point, at which its affinity for caloric will diminish. In most substances this probably does not happen till they reach the point of congelation: but when water is cooled below 36°, its affinity for caloric begins to diminish; the atmospheres, therefore, which surround its particles (and of course the water itself) will expand. Although the particles are nearest to each other at 36°, their attraction is not then sufficient to overcome the resistance of their atmospheres: these therefore will continue to expand, until they are so far rarefied as not to be able to resist the affinity of the particles for each other. But this will not happen till the water is cooled many degrees below the freezing point, unless by agitation, or some other means, they are brought nearer into contact.

Expansion of water by a diminution of temperature.

It was before stated, that the density of the atmospheres was the cause of solidity: but the specific gravity of ice is less than that of water; it also contains less caloric; hence its atmospheres must be less dense; it ought, therefore, to be more fluid than water, which is not the fact. How is this anomaly to be explained? It must be remembered, that not only ice, but all solutions of salts, &c., which form prismatic crystals, undergo the same expansion, during their transition from the fluid to the solid state. It is probably the tendency, which the particles of these bodies have to arrange themselves in prismatic forms, that causes this expansion: and the diminution of specific gravity may be occasioned by interstices between the prisms. In addition

Its expansion on solidification owing to crystallization as is the case with other bodies.

tion to this, it may be observed, that ice always contains a quantity of air. Dr. Priestley ascertained, that, when water, previously freed from air, was frozen in close vessels; it gave out, on thawing, a quantity of azote; and this he repeated several times, on the same water, and with the same effect*. The expansion of iron during solidification depends probably upon the detention of a quantity of caloric between the pores of the metal in an uncombined state, (i. e. not forming part of the atmospheres,) and by hammering, this may be disengaged. Those bodies, which congeal into a shapeless mass, without any appearance of crystallization, contract, as might be expected, during solidification; and this may tend to confirm the explanation given above.

Expansion of iron on solidification.

Repulsion between the particles of caloric an unnecessary assumption.

I have purposely avoided making any mention of the repulsion, supposed to exist, between the particles of caloric; as I believe, that all the phenomena may be explained according to the ordinary laws of affinity. I will endeavour to explain this, in one instance; the expansion of gasses by a diminution of pressure. During the expansion, cold is produced, as was explained before; the caloric, which rushes in, arranges itself around the particles of the gas, according to its affinity for them; and thus removes them to a greater distance from each other. In addition to this, it must be recollected, that the particles will exert an attraction, beyond their own atmospheres, upon those of the neighbouring particles; this will tend to rarefy the atmospheres, and of course to expand the gasses. It is not necessary to suppose, that the particles of water and oil repel each other; if the attraction of the homogeneal particles be stronger than that of the heterogeneal, they will not unite.

No repulsion between oil and water.

Singular phenomenon.

I will now mention a phenomenon, which I once observed, and which, I think, may be explained according to this theory. During the evaporation of a solution of subcarbonate of soda, which had been exposed to a stream of sulphureous acid gas; I observed a number of globules to arise, and run

* This tends to confirm Girtanner's Theory, that azote is an oxide of hidrogen.—[See also the hypothesis of Berzelius, Journal, vol. XXX, p. 270. C.]

about

about upon the surface of the solution. They did not appear to be of a gaseous, but of a liquid nature: they were exceedingly spherical, and their specific gravity so small, that they scarcely sunk at all into the liquid. The bubbles of steam, which were abundant, always receded from them; so as to leave a considerable space around them in every direction. Dr. Higgins observed a similar phenomenon, during the distillation of a mixture of sulphuric and acetous acid. I will give the description and explanation of it, in his own words.—"When the quantity of acetous acid "amounted to about three ounces, and rose in the receiver "so high, that the distance between its surface and the "nozle of the long slender neck of the retort did not "exceed a quarter of an inch, and when the drops fell at "the interval of three or four seconds, each of them, re-"bounding after the fall, and still preserving the globular "form, rolled on the acid liquor; and then, after floating "in a quiescent state for five or six seconds, burst sud-"denly, and spread upon it. Some of these globules in "their motion struck the preceding ones, and frequently "the motion was communicated without either of them "bursting for some seconds after they became quiescent. "Afterward, when the acid in the receiver rose, and was "not distant from the nozle of the retort by more than the "diameter of a large drop, the liquor, which trickled down, "did not discharge itself into the acid, either in the fore-"going maner, or in that of a column; but it formed glo-"bules of six times the former bulk, each of which, pre-"serving its proper form, sunk by half its diameter into "the acid liquor, without mixing with it; but when the "size of any globule increased so as to exceed $\frac{3}{10}$ of an "inch in diameter, it then parted from the nozle, and "spread on the liquor. All this looked as if each drop car-"ried its proper atmosphere of repellent matter, which it "retained for a considerable time, with a force greatly su-"perior to the weight of a grain, for the drop could not, "by reason of its mere aggregation, rebound from liquor "of the same kind, and roll on it, and dip and swell in it "without mixing." Dr. Higgins could not produce the same appearance, in the distillation of water, or of spirit of wine.

A similar phenomenon observed by Dr. Higgins.

I will

Nitrogen expelled from the lungs in respiration.

I will add one more extract from Dr. Higgins's work, which is remarkable, as it confirms the results of the experiments of Messrs. Allen and Pepys, in the respiration of oxigen; and the conclusions which they have drawn from them. After describing the method which he employed to respire the gas, &c.; he adds, "I do not hesitate in con- "cluding, that the former (azote) was expelled from the "lungs during the respiration, along with the matter, "which contributed to the formation of the fixable air."

I am sorry that it is not in my power to digest this paper into a more intelligible form: but it is not more than ten days since I met with Dr. Higgins's book; and it will be a considerable time before I shall have leisure to resume the subject. It has extended to a much greater length, than I at first intended: I am however conscious, that much more remains to be said, and many objections to be answered; but I am not aware of any, which may not receive a satisfactory answer, according to the principles already laid down. The importance of the subject must be my apology, for sending it in this unfinished state; and if it should be the means of drawing the attention of some able chemist to Dr. Higgins's work, and even in this remote way contribute to the improvement of the science, my intention will be fully answered,

I am, sir,

Your obliged and constant reader,

L. O. C.

Coagulation of albumen.

P. S. It may be thought, that the coagulation of albumen by heat is inconsistent with the foregoing theory. To satisfy myself of what actually does take place, during its coagulation, I poured into a glass tube a certain portion of the white of an egg, (which is albumen as pure as it can be procured,) and with a diamond marked the height to which it rose in the tube. The tube with its contents was now plunged into warm water, and the whole apparatus set upon the fire. As soon as the albumen was heated only a few degrees, I could observe bubbles of gas separating from it very abundantly; and before it had begun to coagulate, it had assumed the sparkling appearance of water impreg-

nated

nated with carbonic acid. After the water had boiled for some time, the albumen appeared perfectly white, and was nearly solid; (it does not become perfectly so till cold). In this state it was removed from the fire, but I was surprised to find its bulk increased, rather than diminished, for its surface was very globular. I had not however kept it out of the water more than a minute, before it had sunk considerably, so that its surface was now become concave. If in this state it had been suffered to cool, it would certainly have occupied considerably less space, than when fluid; notwithstanding the extrication of the bubbles of gas, which never rose to the surface, but always continued mixed with the albumen. When it had cooled only a few degrees, it was replaced upon the fire; its surface soon rose again, and by continuing the heat, at last burst, and a quantity of vapour made its escape. The albumen was now full of holes, occasioned either by the separation of gas, or of aqueous vapour. From the experiments of Dr. Bostock it appears, that the white of egg contains 80 per cent of water. Now when this circumstance is also taken into consideration, I think no one would urge the coagulation of albumen by heat as an objection to the foregoing theory. From the experiments which I have just related, it appears exceedingly probable, that albumen contracts during solidification, as is the case with most other bodies; and at any rate, the extrication of gas, and the quantity of water which it contains, will easily account for the apparent anomaly.

It may however still be objected, If heat causes other bodies to become fluid, how can it be the cause of the coagulation of albumen? I *might* reply, that the density of the atmospheres, which surround the particles of albumen, is too great, to permit the particles themselves to approach each other sufficiently near to produce a solid; but that by the addition of caloric, these atmospheres are equalized, and thus the resistance to the motion, (and consequently to the union,) of the particles is removed, or at least diminished. I do not however urge this reason, because I believe it to be far more probable, that the albumen undergoes some chemical change, by the application of heat; and I think, Objection from it. Answered.

think, that Mr. Hatchett has clearly proved, that coagulated albumen is possessed of chemical properties essentially different from those which albumen possesses in a fluid state.

Mr. Dalton's theory.

I perceive from a note in Dr. Thompson's System of Chemistry, that Mr. Dalton supposes the particles of gasses, to be surrounded with atmospheres of heat; but, as I have never seen Mr. Dalton's work, I cannot pretend to say how far he may have anticipated any thing which I have said. Dr. Higgins is undoubtedly the author of the theory, and I have endeavoured to extend it to all the chemical phenomena, which recurred to my recollection.

Mr. Gough on the elasticity of caoutchouc.

From the same excellent work, (Dr. Thompson's Chemistry) I have become acquainted with Mr. Gough's experiments on the elasticity of caoutchouc. When I wrote the paper above I was reading this work, but had not read so far as the part which gives an account of these experiments*: I was therefore very agreeably surprised, to find the same conclusion drawn from experiment, which I had previously deduced from theory, viz: that caloric is the cause of elasticity.

Effect of ignition.

Many substances (*e. g.* carbon) will not combine with oxigen, until ignited; it is perhaps needless to observe, that the addition of caloric equalizes the density of the atmospheres, which surround the particles of these bodies, and that this is the cause of their combination.

Increase of gravity of potassium when oxided accounted for.

It has been objected, by the French chemists, to the theory of Dr. Davy, respecting the metallic bases of the fixed alkalis, &c.: that, if potash (for example) were an oxide of potassium, its specific gravity, like that of all other metallic oxides, should be less than that of the metal, from which it is formed. Not to insist upon the circumstance, that potash is a hydrate of the oxide of potassium, I think it may be clearly conceived, according to the principles stated above, that oxigen may be so far condensed by the abstraction of caloric, as to increase the density of potassium, (or perhaps even of any other metal,) considerably. Accordingly we find, from the experiments of Gay-Lussac and

* This postscript was sent some weeks after the letter to which it is annexed. C.

Thenard

Thenard themselves, that potassium decomposes almost all the metallic oxides with inflammation. Perhaps this increase of specific gravity, which potassium undergoes, may be farther explained as follows. It was remarked by Messrs. Gay-Lussac and Thenard, in the experiments just alluded to, that potassium decomposes the black oxide of iron without inflammation. Let it be granted, that, during the combination of iron with oxigen, the latter only is condensed; and that the metal suffers no condensation*; and it may, I think, be easily shown, that the specific gravity of potash must exceed that of potassium; supposing the oxigen to be in the same state of condensation, as in the black oxide of iron. The specific gravity of iron is 7·8; and that of the black oxide, as nearly as I can determine it, is 4·5.—Now if the black oxide be composed, as Dr. Thompson has shown, of 78·5 iron, and 21·5 oxigen, it may be ascertained, by a very easy calculation, that the sp. gr. of the oxigen, as it exists in black oxide, is nearly 1·7.—If then we suppose, that the oxigen undergoes no farther condensation, when it combines with potassium, (which it certainly must undergo, or it would not decompose the black oxide) still it is evident, that a mere mixture of potassium, of the sp. gr. 0·6, and of oxigen in this condensed state, must be of a greater sp. gr. than the potassium alone. I have stated this, merely to show the possibility of the case, without reference to any theory whatever; but when we add the consideration, that the oxigen is still farther condensed, and that potash is combined with water, every difficulty must be entirely done away.—Permit me to suggest, that, as in alkalis the sp. gr. of the oxigen always exceeds that of the metal, and in oxides falls short of it; this may possibly be the cause of their possessing such different properties, and, in fact, constitute the difference between them.

The difference between oxides and alkalis owing to the condensation of the oxigen.

L. O. C.

* I think no one will refuse to grant this; for it is scarcely conceivable, that the sp. gr. of iron is greater in the black oxide, than in the metallic state.

V.

V.

On the Prevention of Damage by Lightning. In a second Letter from Mr. BENJAMIN COOK.

To Mr. NICHOLSON.

DEAR SIR,

Conductors of lightning.

IN a former paper, which you inserted in your very valuable Journal*, on the advantage and security that I supposed the nation would enjoy, if electric rods were placed at certain distances on the most elevated parts of the country, or if attached to the highest buildings at different places, so that the electric fluid might be carried off by the rods, as the clouds charged with the fluid passed over them; by your remark at the close of that paper, it did not seem to strike you, as promising that advantage and security it did me, and you named an instance, where the rods had failed: But if one instance, or two, have happened, where the electric rods do not appear to have had any influence on the electric fluid, so as to carry it off without injuring the buildings, this is no proof of their inutility. We ought, before we pass judgment upon them, to have known the state of the rods, and their elevation. It is very probable, that these rods had been up for many years, and nearly destroyed by rust; and perhaps in some parts the nature of the iron might have been completely changed or destroyed, and nothing left but rust; nay in some places, even the rods might have been divided, or nearly so, by the rust; so that a weak discharge of the electric fire would easily melt what was left; or they might have been carried in such directions across, or down the sides of the building, as to pass by substances possessing greater power to carry off the fluid, than such rusty decayed conductors; and thus the lightning might have been by their means conducted so as to cause the very ruin, they were intended to prevent.—Besides, the points of the conductors might have been placed very low, so that clouds overcharged with the electric

Occasional failure no proof of their inutility.

Probable causes of failure,

or even mischief from them.

* Vol. XXIX, p. 305.

fluid

fluid might have passed so near the buildings, that every part that was a conductor drew down the fire, as soon as the rods, which had lost a part of their power by rust. I say all this might have been the case, and we therefore ought not to say, that electric rods have been found ineffectual to ward off destruction.

The subject merits investigation.

I am desirous this subject should be fairly investigated, indeed it is a national concern, and I do wish some able person would take up the subject; and if any of your correspondents could produce any one instance, where the rods, having been found in proper order and position, have failed, it would in a great measure prove their inutility. On the other hand, if any one instance could be brought forward, where they have proved beyond a doubt the protectors of a building, that without them would have suffered, some basis might be laid down to form a just idea upon.—This is certain, that we have each year to record great losses, both in property and lives, by the electric fluid; and if some plan could be devised, to remedy, if but in part, the evil experienced and complained of, great advantage and safety would be procured to society.—My opinion is, that electric rods are sure and certain preservatives to every house, where they are properly attached, if of the proper kind; and if a house can be secured, why not by the same *means* a whole parish, by a proper number of conductors?

Conductors in general defective.

But conductors are of little or no use made in the way they commonly are, of a piece of iron wire one quarter of an inch in diameter, or perhaps less; for in many I have examined they have not been so thick, some merely a strong wire. These in a year or two are nearly or quite corroded through with rust; and they are attached in a careless way, with a number of rusty points at top, directed to every point of the compass, and rising just above the chimney. It appears, that, if a rod is placed against a house or building, no matter how, the building is supposed to be safe; and if this house or building is injured by lightning, it is the rod that was to have protected it, that is declared inefficacious. These rods are generally put up by some carpenter, or builder, who knows nothing of the nature or properties of the fluid he is guarding against, and therefore brings the rod

rod down any way that is most convenient, without considering whether it passes near or even touches any conducting substance in the building; in which case the rod, instead of protecting, is calculated to bring on the building the mischief it was intended to prevent.

How they ought to be made and applied.

Electric rods should be three quarters of an inch in diameter, according to my opinion; should not touch the building in any part by three inches; and all their fastenings to it should be by nonconductors. They should end in a single point of brass, and this point be elevated six feet at least, but ten feet if possible, above the highest chimney of the house. If the rod is not of brass, or a tube of brass, a strong brass wire ought to be wound round it, connected with the point, and passing once round the rod in the space of 12 or 18 inches, sufficient to keep the brass wire close to the iron, all down to the earth. I have no doubt upon my mind, from all the observation I have made, that electric rods of this nature will never fail to give perfect safety. Even on board vessels an iron chain, the worst of all conductors that can be called a conductor, has been known to preserve the vessel and crew. As a proof, I will quote a passage from Captain Cook's Journal of his Second Voyage Round the World. "April 25, 1774. "Otaheite—This day we had a very violent tempest. We "were obliged to get our electrical chain up to the top- "gallantmast head, to secure the masts. Removed all "the iron off the decks, and secured down all the hatches. "—As the seaman, who carried the chain up, was "coming down, he got foul of the chain, and it lightning "at the same time, he received a slight blow on the leg, "which, though it did him no harm, shook every bone "within him." Captain Cook had seen an instance of the great utility of the electrical chain in his former voyage, while at Batavia, which, being of a singular nature, I shall relate in his own words, or as they are given by Dr. Hawkesworth. "About 9 o'clock we had a dreadful storm of "thunder, lightning, and rain, October 10, 1770, during which the mainmast of one of the Dutch East "Indiamen was split, and carried away by the deck: "The main topmust and topgallantmast were shivered all

Instance of security.

Another.

"all to pieces; she had an iron spindle at the main top-"gallantmast head, which probably directed the stroke.—"This ship lay not more than the distance of two cables "length from ours, and in all probability we should have "shared the same fate, but for the electrical chain, which "we had but just got up, and which conducted the light-"ning over the side of the ship. But though we escaped the "lightning, the explosion shook us like an earthquake, the "chain at the same time appearing like a line of fire: a "centinel was in the action of charging his piece, and the "shock forced the musquet out of his hand, and broke the "ramrod.—Upon this occasion I cannot but earnestly re-"commend chains of the same kind to every ship, whatever "be her destination; and I hope that the fate of the Dutch-"man will be a warning to all, who shall read this narra-"tive, against having a spindle at the mast head."

Thus even chains have been found protectors, and if proper conductors were attached to the main topgallant-mast, running all down it with a joint at the place where the mast is jointed, it would always be in its place; and I again say, I am pretty confident, that vessels would be secured from the injury they but too often sustain from lightning, as well as houses.—The rod would not be in the way of any of the rigging, and therefore I should think it would be a duty the masters of vessels owe to their sailors, as well as to the owners of the property they have on board, to be always provided against danger. I am, dear sir,

Rods recommended for ships instead of chains.

Your obedient servant,

B. COOK.

BIRMINGHAM,
Caroline Street, Dec. 27, 1811.

VI.

Observations on some of the Strata in the Neighbourhood of London, and on the Fossil Remains contained in them: by JAMES PARKINSON, *Esq., Member of the Geological Society.*

(Concluded from p. 52.)

Strata interposed between the Clay and Chalk.

IT is almost impossible to speak with precision of the subjacent strata, which are situate between the clay and the chalk,

Strata beneath the clay.

chalk, since very considerable variations occur as to their thickness, and indeed as to the form in which their constituent parts are disposed; and since there exist but few sections, at least in the neighbourhood of the metropolis, which present a view of the strata composing this formation. They are included in the following account by Mr. Farey: "A sand stratum, of very variable thickness, next succeeds, "and lies immediately upon the chalk, in most instances, "as between Greenwich and Woolwich, on the banks of "the Thames; which has often been called the *Blackheath* "*sand*, it frequently has a bed of cherty sandstone in it, "called the gray-weathers"*.

The bottom of the clay

On the upper part of a mound at New Charlton some traces of the lowest part of the blue clay appear, covered by not more than a foot of vegetable earth. This layer of clay does not seem to exceed two feet in thickness, which, indeed, it possesses only on the top of some of those mounds, which occur so frequently as to render the surface in this district very irregular. In this clay oysters of different forms are found: some approaching to the recent species, and others longer and somewhat vaulted; but they are in general so tender, as to render it very difficult to obtain a tolerable specimen. With these also occur numerous *cerithia*, *turritellæ* and *cythereæ*, Lam.; all of which are in a similar state with the oysters, and appear to be shells strictly belonging to the subjacent stratum, but which, having lain uppermost, became involved in the first or lowest deposition of the blue clay.

contains shells

belonging to the stratum beneath.

Shells in great measure disintegrated.

Immediately beneath the clay there is found a line of about three or four inches of the preceeding shells imbedded in a mass of calcareous matter, the result of their disintegration. Beneath this are numerous alternating layers of shells, marl, and pebbles, for about twelve or fifteen feet. The shells are those which have been already mentioned; but are very rarely to be met with whole, and when entire are so brittle as to be extricated with much difficulty. In some of these layers scarcely any thing but the mere fragments of shells are to be found, and in others a calcareous powder only is left.

* Report on Derbyshire, &c. vol. I, p. 111.

The

The pebbles are almost all of a roundish oval form, many of them being striped, but differing from those of the superior stratum, in being seldom broken, in there being few large ramose masses, and in their not bearing any marks or traces of organization. Many of these pebbles are passing into a state of decomposition, whence they have in some degree the appearance of having been subjected to the action of fire: small fragments of shells are every where dispersed amongst them. Pebbles.

Beneath the pebbles is a stratum of light fawn coloured sand of about ten feet in depth, and immediately under this is the stratum of white sand, which is about five and thirty feet deep, and is here seen resting immediately on the chalk. Sand beneath these.

At Plumstead, about a mile distant in a south-eastern direction, there is a pit in which the shells, about two years ago, were to be obtained in a much better state of preservation than at New Charlton; but this seam of shells, as the pit has been dug farther in, has by degrees become so narrow, as to be now nearly lost. In this pit, not only the shells already mentioned were found, but many tolerably perfect specimens of *calyptræa trochiformis*, Lam., *trochus apertus*, Brander., *arcæ glycemeres*, *arcæ Naticæ*, and many minute shells in good preservation. All these shells appear to have entirely lost their animal matter, and, not having become imbued with any connecting impregnation, they are extremely brittle. On examination with a lens it also appears, that in most of the specimens nothing of their original surface remains, it having been every where indented with impressions of the surrounding minute sand, made while the shells were in a softened state. This circumstance is particularly evinced in the *cyclades*, in which a particular character in the hinge was thus concealed; in a mass of these shells from the Isle of Wight, it appears, that the lateral teeth are crenulated, somewhat similar to those of the *mactra solida* in the gravel stratum; but in the *cyclades* of Plumstead, this was not discoverable from the injuries, which their surface had sustained from the sand. Shells at Plumstead. Particular character in the hinge of one concealed.

The fossils of this stratum evidently agree with those found by Lamarck and Mr. De France, above the chalk at Grignon, Courtagnon, The same fossils found in France, and in

the Isle of Wight.

Courtagnon, &c.; and they have been just shown, incidentally, to exist in the Isle of Wight. In an eastern and southern direction from London this stratum with its fossils is frequently discovered.

Shells about Crayford.

On the heath near Crayford, about four miles eastward of Charlton, long vaulted oysters are found similar to those already mentioned. About two miles farther, in the parish of Stone, is *Cockle-shell-bank*, so called, as Mr. Thorpe, the author of *Custumale Roffense*, says, p. 254 of that work, "from "the great number of small shells there observable." These are the *cyclades* already spoken of, and which Mr. John Latham, author of *The general Synopsis of Birds*, thought bore some resemblance to *tellina cornea*, Linn., *Histor. Conchyl.* of Lister, tab. 159, fig. 14. Mr. Latham here also met with a species of *cerithium*, and another of *turritella*. Fragments of these shells are also frequently turned up with the plough in that neighbourhood. They have likewise been found at Dartford, at Bexley, and at Bromley, to the southward.

Large mass of stone filled with shells.

Mr. Thorpe also relates, that, in the parish of Stone, there was a large mass of stone, of some hundreds weight, full of shells, which was brought from a field, and used as a bridge or stepway over a drain in the farm-yard. (*Custumale Roffense*, p. 255.)

Course shelly limestone.

In several spots in the neighbourhood of Bromley, stone is found near the surface, formed of oyster-shells, still adhering to the pebbles to which they were attached, and which are similar to those which have been just described, as occurring at Plumstead and at Charlton: the whole being formed by a calcareous cement into a coarse shelly limestone containing numerous pebbles. The only quarry of this stone, which has been yet worked, is in the grounds of Claude Scott, Esq. The opening hitherto made is but small; it is however sufficient to show, that the stratum here worked has suffered some degree of displacement, as it dips with an angle of about forty-five degrees.

Stratum of sand over the chalk,

At Feversham, over the chalk, Mr. Francis Crow has discovered a bed of dark brown sand, slightly agglutinated by a siliceous cement, and intermixed with a small portion of clay. In this stratum, which has been hitherto but little explored,

he

he has found, in a siliceous state, specimens of *strombus pes pelicani*, and a species of *cucullæa*, nearly resembling those which are met with in the Black-down whetstone pits.

Patches of plastic clay are frequently found over the chalk: some of these are yellow, and employed for the common sorts of pottery; but others are white, or grayish white, and are used for finer purposes. The coarser clay is very frequently met with, nor are the finer kinds of very rare occurrence. In the Isle of Wight two species of plastic white clay are worked for the purpose of making tobacco-pipes. A similar clay, which is used for making gallipots, is dug from the banks of the Medway. A fine light ash-coloured, nearly white clay, which is employed in pottery-works, is also dug at Cheam near Epsom in Surry.

and frequently patches of potter's clay, of different kinds.

The *upper* or *flinty chalk*, which is the next older stratum, is extremely thick, forming stupendous cliffs upwards of six hundred and fifty feet high, on the south-eastern coasts of the island. It extends nearly through almost all that part of the island, which lies south of a line supposed to be drawn from Dorchester in the County of Dorset to Flamborough-head in Yorkshire.

Upper or flinty chalk, a very thick stratum.

In this stratum there is a great quantity of flint, chiefly in irregularly formed nodules, disposed in layers, which preserve a parallelism with each other and with continuous seams of flint, sometimes not exceeding half an inch in thickness. The chalk contains a fine sand, which may be separated by washing*.

The flint in parallel layers.

The fossils of this stratum are for the most part peculiar to it; very few of them being found in any other. They also appear to agree very closely with those species found in the chalk of France, by Messrs. De France, Cuvier, and Brongniart. The number of fossils noticed by these gentlemen amounts to fifty; but they have yet only particularised a part of them. These are here compared with what appear to be the correspondent fossils in the English part of this stratum; and some others are also pointed out, which these gentlemen have not yet mentioned as being found in the neighbourhood of Paris.

Fossils of this stratum chiefly peculiar to it. Closely allied to those in France.

* The chalk in the neighbourhood of Paris contains, according to Mr. Bouillon La Grange, magnesia 0·11, and Silex 0·19.

In the French stratum there occur,

Fossils in the French stratum.

Two *lituolites.* No species of this genus is noticed as having been seen in our English chalk. But research has not been made with the necessary precision.

Three *vermiculites.* The fossil figured Org. Rem. vol. III, pl. VII, fig. 11, was considered as a vermiculite, until by removal of the chalk, and opening different specimens, it was found to be a chambered and an adherent shell. Should these gentlemen not have perceived these circumstances in the specimens they met with, they would certainly regard this fossil as a vermiculite. It must also be observed, that, from the different forms in which the spiral part is disposed, its division into two or three species might be authorised.

Belemnites. These, according to Mr. De France, are different from those which accompany the *ammonites* of the compact limestone. The *belemnites* of our chalk are smaller than those of the limestone, beside which they are different in form, being narrower and more elongated. But Mr. De France may also have confounded with them the spines of the *echinus,* which so closely resembles the *belemnite*: if that gentlemen should not have met with perfect specimens, he might not be able to remark the difference between these two fossils. The characters, which he has noticed, are however sufficient to lead to the belief of a correspondence between the French and English fossils.

Fragments of a thick shell of a fibrous structure. The doubts expressed respecting the nature of this shell, and the observations made with regard to it, offer another strong point of agreement between the shells of the two strata. The shell here alluded to is most probably that represented Org. Rem. III, pl. V, fig. 3; the structure of which agrees exactly with that mentioned as found in the French stratum of chalk. That shell is however described as being of a tubular form; it is therefore right to observe, that fossil *pinnæ* do sometimes possess this peculiar structure.

A *muscle.* No instance appears in which any shell of this genus has been found in our chalk.

Two *oysters.* The Kentish chalk-pits yield at least three species of this genus. One of them bearing very much the form and appearance of *ostrea edulis,* but being only about a fourth

a fourth of its size; one smaller, the serrated edge of which places it in the family of *cristæ galli*; and the third still smaller, not half an inch in length, crenulated on each side of the hinge.

A species of *pecten*. There are two or three small species of *pecten* in the English chalk; beside a shell with long slender spines, which may be safely classed with the pecten.

A *crania (anomia craniolaris*, Linn., *crania personata*, Lam.). This fossil is not known in the English chalk; nor indeed could it be easily ascertained, unless the inferior valve happened to be well displayed.

Three *terebratulæ*. *T. sulcata* and a terebratula agreeing with *anomia terebratula* Linn. are frequently found in our chalk; and sometimes another species, hardly half an inch in length, with remarkably acute and well defined ribs.

A *spirorbis*. Traces of these shells are frequently found on the surface of the *echinitæ*.

Ananchitæ, (echinus ovatus), the crustaceous covering of which, it is remarked by Messrs. Cuvier and Brongniart, remains calcareous, and has assumed a sparry texture, while the middle alone is changed into silex. No actual change has however taken place. as far as respects the flinty part of the fossil, the flint having merely filled up the hollow of the sparry crustaceous covering, This fossil is frequently found in the English chalk.

Porpitæ. These also occur in the English chalk.

Five or six different fossil bodies called by the French oryctologists *polypiers*, one appearing to belong to the genus *caryophyllæa*. Several of these bodies, from the English chalk, have been figured in the Org. Rem. vol. II, Pl. XIII, fig 70 to 79.

Another is supposed to belong to the genus *millepora*. This is generally brown, and is in the state of oxidized iron, as resulting from the decomposition of pyrites. These fossils exist in the Wiltshire soft chalk.

Lastly, *shark's teeth*. These also occur frequently in the English stratum.

Messrs. Cuvier and Brongniart state, that there are many more fossils in the chalk stratum of France, than those which have been just referred to. This is also the case with the

Many more fossils in the French stratum, and in the English.

the fossils of the English chalk; since the following may be enumerated as occurring in this stratum. *Rugous palates*, and though rarely, the *scales* and *vertebræ of fishes*. Three or four species of *stellæ marinæ*. *A long saccular bivalve*, with an uncommonly thin shell, of which so little has been hitherto saved, as not to give a chance of gaining a knowledge of its general form or the structure of its hinge. *A bivalve*, which approaches to a circular form, but is so thin as to afford but little hope of discovering its genus. *A bivalve, nearly circular, the margin turning upwards so as to give it a patella or disk form, with numerous long processes passing from the margin and external surface, and fixing it to other bodies. A small pecten with sharp angulated ribs*, not exceeding a quarter of an inch in length. *A bivalve*, not an eighth of an inch in length, finely striated longitudinally, bearing a bright polish, and seemingly possessing its original light brown colour. *Plates of the tortoise echinite*, and several remains apparently of other species of this genus.

When to these are added the remains of various *echini*, such as *conulites*, *cassidites*, and *spatangites*, and the different *spines* of *echini* which are found in this stratum; and when it is also considered, that the present account is drawn up almost entirely from the productions of chalk cliffs of not more than two miles in length, it will not be difficult to conceive, that the number of these fossils is not less in the English than in the French chalk.

These fossils imbedded by a gradual deposition.

The state, in which these fossils are found, plainly evinces, that the matrix in which they are imbedded was formed by a gradual deposition, which entombed these animals while living in their native beds. The fine and delicate spinous projections of the shells are unbroken, and the spines are still found adhering to the crustaceous coverings of the *echini*; neither of which circumstances could have occurred had these bodies been suddenly and rudely overwhelmed by these investing depositions, or had they been brought hither from distant spots.

Objection answered.

It may be said, that the specimens possessing the characters here alluded to are rare. With respect to the spinous shells, however, they certainly occur often, although it is almost impossible to extricate them unbroken from their surrounding

colating fluid, by the crystallization of which these bodies have been formed, and are now augmenting.

The Oberstein nodules of agate appear to have been formed under somewhat similar circumstances; since it is in general evident from their external surfaces, that they also have had very little adherence to their matrices, which would hardly have been the case, had these been highly impregnated with silex. Oberstein nodules of agate.

The hard chalk lies immediately beneath the soft chalk. In this stratum there are no flint nodules. "Its beds," according to Mr. Farey, "increase in hardness, until near "the bottom, where a whitish freestone is dug, at Tottern-"hoe in Bedfordshire, and at numerous other places; that "brought from Ryegate and other quarries of this stratum, "south of London, is used as a fire-stone*." Hard chalk without flint.

It has been generally supposed, that these two strata of chalk are of one formation: but not only the absence of the flints, but the characters of their fossils, prove them to be of distinct formations. No fossils indeed are marked by more decidedly peculiar characters than those of this stratum; since hardly a single fossil has been found in it, which has been met with in the soft chalk, or any other stratum. A different formation. Its fossils peculiar.

It is in this chalk, that the genus *ammonites* is first met with; or, in other words, it appears, that the water, which formed this stratum, was that in which this genus last existed, no traces of it having been seen in the soft chalk, or in the other superior strata. The chief, and perhaps the only circular species of this genus, which has been found in this stratum, is of a large size, with nodular projections on its sides, toward the back, which is generally flat. This fossil appears to be of a different species from any of those, that are found in the subjacent strata. Ammonites first occur in it. One species found in no other.

It is very remarkable, that in this stratum, the last in which the genus *ammonites* is met with, so remarkable a deviation from the original form of the genus should occur, as almost to claim its being considered as the characteristic of another genus. In the fossil here referred to, which pos- One with almost a generic difference.

* Report on Derbyshire, &c., p. 112.

sesses

sesses all the other characters of *ammonites*, the spiral coil is disposed in a form rather approaching to that of the oval than the circle*.

A still more remarkable deviation.

In another fossil of this stratum a still more extraordinary deviation exists. This fossil possesses the concamerations and the foliaceous sutures of the cornu ammonis; but, instead of being spirally coiled, it has its ends turned toward each other, somewhat in the form of a canoe. This peculiar form has led to the placing of this fossil under a separate genus, which has been named *scaphites*†.

Extent of this stratum.

Of the extent of this stratum no correct account has been given; but there is sufficient reason for believing, that it accompanies the other chalk in its range through this island. It also appears, that its peculiar fossils exist in it at very considerable distances. Thus the *oval ammonite*, which is found in the Sussex hills, likewise occurs in the hard chalk of Wiltshire; and the *scaphites*, another inhabitant of the Sussex hills, has also been discovered in Dorsetshire.

The strata above the chalk in England differ from those in France.

On comparing the preceding sketch with the Essay on the Mineralogical Geography of the neighbourhood of Paris, by Messrs. Cuvier and Brongniart, some important variations will be perceived between the strata found above the chalk in this island and in France. In France, the strata above the chalk differ both in number and quality from those, which have been hitherto observed in a similar situation in England. In France too, several strata of sand and sandstones exist above the strata of the gravel formation, which in this island appear to be highest.

Attempt to account for this.

The first of these differences appear to result chiefly from the existence of numerous beds or patches, the formation of which must have depended on certain local circumstances, such as the existence of fresh or salt water lakes, at the period of the drying up of a former ocean; the different chemical combinations, which might thence have taken place; &c. But the occurrence of such variations can hardly be considered as interrupting the continuity of the stratification.

* Organic Remains, vol. III, pl. IX, fig. 6.

† Organic Remains, vol. III, pl. X, fig. 10 and 11.

Indeed

Indeed when it is considered, that in France much more frequent opportunities are afforded of examining the stratification immediately above the chalk than in England, it will not be regarded as improbable, that several of these beds or patches may exist here, the discovery of which would render the accordance of the two series of strata much more close.

Even from the examinations, which have been already made, the identity of the French and English chalk is established. The British strata above the chalk are also found to contain patches of plastic clay, of most of the varieties mentioned in the French strata, as well as patches of coarse limestone, with its accompanying sand and its peculiar fossil shells, such as are found to exist in the corresponding French strata.

The other difference, the existence, in France, of beds of sand and of sandstone above those of gravel, which are the highest strata of this island, is very remarkable. May it not be attributable to the abruption, from this island, of the superior strata or beds of this formation, by that catastrophe, instances of the astonishing force of which have been already noticed?

VII.

Experiments on Muriatic Acid Gas: by J. MURRAY, *Lecturer on Chemistry, Edinburgh.*

To Mr. NICHOLSON.

SIR, *Edinburgh, Dec.* 28, 1811.

THE state of my health has not allowed me to send you an earlier account of the farther experiments on the nature of muriatic and oximuriatic acids, which I announced in my last communication. I now beg leave to submit them to the attention of your readers. **Reason of delay.**

I have already observed (Journal, vol. XXVIII, p. 139,) that there are two modes of investigation, by which the question at present under discussion with regard to the nature **Two modes of deciding the nature of oximuriatic acid.**

nature of the relation between muriatic and oximuriatic acids may be determined. Either it may be shown, that oximuriatic acid does or does not contain oxigen; or it may be proved, that muriatic acid gas does or does not contain water.

Direct mode.

If it be proved, that oximuriatic acid contains oxigen, then it must be regarded as a compound of that element with muriatic acid, and the discussion is at once terminated.

Indirect, from oximuriatic acid and hidrogen forming muriatic acid and water.

The other mode, though less direct, is equally conclusive. In the experiment of the mutual action of oximuriatic gas and hidrogen gas, muriatic acid gas is the sole product. Mr. Davy regards it as a compound formed by their union; and, if it can be shown to be the real acid free from water, or any other ponderable matter, this is the conclusion, which appears necessarily to follow. But, if muriatic acid gas contain water, the conclusion is inadmissible; the origin of this water must be accounted for; and there is no other mode of doing this, but by the established theory, that oximuriatic acid is a compound of muriatic acid and oxigen; and that, in its action on hidrogen gas, its oxigen combines with the hidrogen, forming water, which the muriatic acid, its other element, holds combined with it in the gaseous form. The proof therefore of the existence of water in muriatic acid gas is a conclusive proof of the truth of that theory, and at the same time a demonstration of the falsity of the opposite hypothesis. My former experiments were designed to gain proof of the existence of oxigen in oximuriatic acid: those which I have now to state were undertaken with the view of obtaining evidence of the existence of water in muriatic acid gas.

Attempts to establish this.

Difficulty in the experiments admitting of explanation either way.

The difficulty is to find in this mode of investigation an experiment, which shall be conclusive. Such is the facility with which both hypotheses may be adapted to the phenomena, that there is scarcely a case of chemical action exerted either by muriatic or oximuriatic acid, in which an explanation may not be given in conformity to the one as well as to the other. And although the explanations afforded by the common system are less complicated than those of the other, and are more conformable to analogy from similar

lar cases of chemical action exerted by other acids, yet still, since a possible explanation may be given by the latter, the question remains so far undecided.

This observation applies to the experiments, from which it was inferred, that water exists in muriatic acid gas; though at first view they appear to prove it, the proof must be admitted to be doubtful, as they admit of explanation on the opposite opinion. Thus the proof from the agency of water in facilitating the expulsion of muriatic acid gas from dry muriates is ambiguous, as the water may be supposed to operate either by its affinity to the acid, or by affording hidrogen to form it. The production of hidrogen, when metals are acted on by muriatic acid gas, is a proof of equal ambiguity; since it may be supposed to be derived either from the decomposition of the acid, or of water existing in it. Even apparently the most conclusive of all these facts,—the production of water, when muriatic acid gas is acted on by substances with which acids in general combine, as for example the metallic oxides, admits of this double explanation. The acid is absorbed; and it might be inferred, that it combines with the metallic oxide, while the water which appears is deposited from the gas, in which it had previously existed in a state of combination. But this conclusion, though conformable to the most extensive and strict analogy, is avoided on the opposite hypothesis of muriatic acid gas being a compound of oximuriatic acid and hidrogen, by the supposition, that the acid is decomposed, that its hidrogen combines with the oxigen of the metallic oxide, and forms this water, while the metal itself combines with the oximuriatic acid. Instances of this.

If we can procure however a substance not oxidated, and yet capable of combining with muriatic acid, this source of ambiguity is avoided, and the experiment may be rendered conclusive. There is only one such substance—ammonia. No oxigen can be detected in its composition, and Mr. Davy himself admits, that it combines directly with muriatic acid, and does not decompose it. It cannot therefore cause any formation of water. Neither can it be supposed to afford water; for, when dried by exposure to substances having a strong affinity to water, it retains no sensible portion; Only mode of avoiding this difficulty.

tion; nor is any discovered to have existed in it, when it is decomposed. Its combination with muriatic acid gas is thus calculated, I conceive, to afford what is so desirable, yet difficult to attain in the present question, an *experimentum crucis.* If, on combining dry ammoniacal gas with muriatic acid gas, no water is obtained, the result is so far in conformity to Mr. Davy's theory; and it may be concluded, that the water obtained in other combinations of muriatic acid gas has not preexisted in it, but is directly formed. If, on the contrary, water is obtained; as it does not preexist in the ammoniacal gas, and as there is no such mode of accounting for its production as in those cases where oxigen is present, the water must be inferred to have existed in the muriatic acid gas, and the truth of the common opinion is of course established. To ascertain the fact the following experiments were made.

Experimentum crucis.

Dry ammoniacal gas, combined with dry muriatic acid gas.

Ammoniacal gas was dried carefully by exposing it over dry quicksilver to the action of quicklime. Muriatic acid gas received over dry quicksilver was combined with it, to neutralization: or rather leaving a very slight excess of alkali, to guard more effectually against any excess of acid, which might communicate to the product a slight degree of deliquescence. Thirty cubic inches of muriatic acid gas, and thirty-two cubic inches of ammoniacal gas, were employed. The white spongy salt was collected from the sides of the jar. It gave indications of humidity: for, although the surface of it appeared loose and spongy, it could not be entirely detached from the glass, but adhered slightly to it; in removing it by a knife, it spread a little over the surface, as any substance very slightly moist and clammy would do; and, when pressed together by a knife, its parts adhered slightly. It was put immediately into a small glass retort with a long neck, which was connected with a small receiver having two tubulatures, into one of which the tube of the retort was fitted by grinding, and into the other a long straight tube of narrow diameter, open at both extremities, was inserted. The retort being placed in sand, heat was applied by a lamp. In a short time a thin film of moisture condensed in the neck of the retort, which increased and collected into small globules, which accumulated, and trickled

The product, which had some signs of moisture.

Immediately distilled,

and moisture condensed in the neck of the retort.

trickled down: the heat being applied gently, that the salt itself might not be volatilized, there was no sensible condensation of humidity on the sides of the receiver, or in the tube inserted into it. When the farther condensation of moisture appeared to have ceased, the lamp was withdrawn, the retort was cut, and the residual salt removed; a little of it, which had been volatilized, and formed a thin film on the upper part of the retort, being collected, and added to the other portion. The salt had lost in weight 1·3 gr.—a loss obviously to be ascribed to the expulsion of water, and the quantity condensed in the neck of the retort appeared fully equal to this. This is the smallest portion too, that was obtained in frequent repetitions of the experiment, and in some of these the quantity was equal to 1·5 gr.; a difference depending probably on the temperature applied. 100 cubic inches of muriatic acid gas weighing 39 grains, 30 cubic inches weigh 11·7 gr.; and this affording 1·3 of water gives the proportion of $\frac{1}{9}$ of its weight.

The salt had lost weight, apparently water.

To the amount of one ninth of the weight of the muriatic acid.

It could not be presumed however, that in this experiment the whole water of the compound salt was disengaged. In every case of the combination of an acid with any base, part of the water of the acid enters with it into the combination, at least when the product is a soluble salt, and is not easily entirely abstracted. There is no reason to suppose, that this should not be the case in the combination of muriatic acid and ammonia; and there must be even a greater difficulty in expelling this water from an ammoniacal salt by heat, than from other salts, on account of its volatility. There is another difficulty in the present case; we cannot introduce the affinity of any other substance to the acid, which, combining with it, might allow a portion of the water to be disengaged; for we can employ no substance with this view, but one which is oxidated, and which would therefore introduce a source of ambiguity, as it might be supposed, on Mr. Davy's hypothesis, to *form* water by its action on the acid itself.

This propably not the whole of the water present.

The most direct method of discovering any farther portion of water in the salt, free from this ambiguity, appeared to be to expose it to a red heat in mixture with charcoal; for, although the whole quantity could not be expected to be abstracted

Most direct method of proving this.

abstracted even by this mode, yet a portion might be expelled at so high a temperature, and the charcoal might also by its strong attractions to the elements of water abstract a portion, which would be indicated by the production of its compounds with these elements. The following experiment was accordingly made.

The remainder of the salt exposed to a red heat mixed with charcoal.

Charcoal in powder was exposed in a clear iron tube, the open extremity of which terminated in quicksilver, to a heat gradually raised to a very high degree of intensity; and this was kept up until the production of elastic fluid ceased. The charcoal was allowed to cool in the tube without the admission of air, and, when nearly cold, the salt remaining in the former experiment was mixed with about an equal weight of it. This was put into a Wedgwood's earthenware tube; the tube was nearly filled with the same charcoal, and was placed across a small furnace, and surrounded with burning charcoal, so that the middle of it was raised to a red heat. A sufficient heat was thus communicated to the closed end of the tube to volatilize the ammoniacal salt, and cause it to pass through the ignited charcoal; to the other extremity a bent glass tube was adapted, terminating under an inverted jar filled with mercury in the mercurial trough. Elastic fluid began to come over; this was accompanied with a condensation of water in the curved glass tube; the gas itself very soon came over opaque, and humidity appeared on the sides of the jar, and the surface of the mercury within it. When two jars, containing about 14 cubic inches, had been filled, the gas which came over had become transparent; from 15 to 20 cubic inches were produced. Portions of this elastic fluid exposed to limewater caused a milkiness in it, with diminution of volume; the residual gas, after slight agitation with water, burned with the faint yellow flame of hidrogen, and, after its combustion, rendered limewater slightly milky. The charcoal in the tube being agitated with water, the liquor filtered from it was limpid, it had a strong saline taste, and on the addition of potash or lime exhaled a strong ammoniacal smell.

Water passed over, carbonic acid, and hydrogen: and ammonia left in the charcoal.

Rationale of the experiment.

The rationale of this experiment is sufficiently obvious. From the temperature being much higher than in the preceding

preceding experiment, an additional quantity of water was expelled from the muriate, its separation being aided by the mechanical effect of the charcoal, which, while it impeded the sublimation of the salt through the whole length of the tube, would allow the more highly elastic watery vapour to pass. At the same time a portion of this water suffered decomposition, producing, by combination of its elements with the charcoal, carbonic acid, and carburetted or oxicarburetted hidrogen gas. The quantity of carbonic acid was from 1 to 1·3 cubic inch, estimated from the diminution of volume.

Water in the first stage of it,

In both these experiments then, or rather in these two stages of the same experiment, the presence of water in the compound formed by the union of muriatic acid gas with dry ammoniacal gas is demonstrated. Its disengagement from the salt in the first stage of the experiment was not in the least ambiguous, and the quantity was even considerable in relation to the quantity of acid gas employed, being equal to a ninth of its weight. And, as has been already remarked, this cannot be supposed to be the whole. Had there been no sensible production of water, the presence of any in the gasses combined could not have been inferred; and it could not therefore have been inferred with certainty, that any existed in the concrete salt. But since water was produced, and its existence in one or both of the elements of the salt is thus demonstrated, it is farther certain, if we can rely on any conclusion from the most strict and extensive analogy, that the whole quantity could not be expelled by the heat applied.

and in the second,

In the second stage of the experiment, the disengagement of a farther portion of water was abundantly evident, though, from the nature of the experiment, it was difficult to ascertain its quantity with the same precision. Judging from the appearance of the condensed moisture in the curved glass tube, and in the jars, the quantity was nearly equal to that condensed in the first stage of the experiment; and to this is to be added the quantity decomposed by the ignited charcoal, which formed the carbonic acid and carburetted hidrogen. Adding these, and taking the average of the experiments, I would not hesitate in estimating it equal to

 the

the quantity, which appeared in the first stage of the experiment. This, supposing it to be derived from the muriatic acid gas (and, as has been shown, this can be the only origin assigned so it,) gives 2·6 of water in 30 cubic inches, or 11·7 grains of the acid equal to $\frac{2}{9}$ of its weight. The quantity of carbonic acid, (and this could be estimated with accuracy,) taking it at one cubic inch, contains as much oxigen as is contained in ·5 gr. of water, and this of itself added to the quantity obtained in the first stage of the experiment makes the water amount to $\frac{1}{5}$ nearly of the weight of the acid; with the addition therefore of the moisture visibly condensed in the tube and jars, the quantity cannot be less than between a fourth and fifth of its weight.

making together 0.25 or 0·2 of the weight of the acid.

The production of water at least demonstrated:

It may be remarked too, that, though the quantity obtained in this stage of the experiment may not admit of being estimated with perfect precision, there is no source of fallacy with regard to its production. The charcoal had ceased to give out gas at a heat of much higher intensity than that to which it was afterward exposed in mixture with the muriate; the water therefore, or the elastic fluid obtained, could not have been derived from it; and indeed this water appeared at the very commencement of the experiment, when the heat was scarcely equal to that of ignition. If the charcoal afforded any gas too, it could only be a portion of the carburetted hidrogen, and on the quantity of this produced no stress has been laid in drawing the conclusion from the experiment. And it is to be repeated, that the existence of water in the muriatic acid gas to the extent at least of $\frac{1}{5}$ of its weight is demonstrated in the first stage of the experiment, and that, from what must remain in the compound salt, the quantity must be greater than this.

and the proportion inferred from the above experiments coincides with what had been inferred from others.

Not only is the presence of water demonstrated by this experiment, but the quantity is nearly the same as that indicated by the action of other substances, which are supposed by Mr. Davy to form it by affording oxigen. Thus Gay-Lussac and Thenard have inferred from the action of oxide of silver or of lead on muriatic acid gas, that it contains very nearly a fourth of its weight of water; and the quantity, which may be fairly inferred from the preceding

ing experiments, is nearly the same*. Although it is not necessary, that the quantity should be proved to amount to this, to refute Mr. Davy's hypothesis, and establish the common theory, yet it is satisfactory to have this coincidence. And it must be farther admitted as a proof, that the oxigen of these oxides has no share in the production of this water: for it is obvious, that, were the water, which is deposited when muriatic acid gas acts on metallic oxides, on the fixed alkalis, or the earths, formed by the oxigen of these substances, and not derived from the gas as previously existing in it, there can be no production of it in the mutual action of muriatic acid gas and ammonia, as ammonia cannot afford oxigen. Since it is produced in that action it must be derived from the muriatic acid gas, and the same origin must be assigned to it in the other combinations of this acid.

The experiments conclusive,

This experiment then has the advantage of being conclusive on the subject of the present discussion; the state of the fact only requires to be ascertained, and with due precaution this is not difficult of attainment. There is at least no mode of accounting for the production of water, but by assumptions so gratuitous and unfounded, as to be equal to the refutation of the theory. Such is the only assumption that can be made—that the water may be derived from the ammoniacal gas, and not from the muriatic acid gas. When ammoniacal gas is dried by potash or lime, no water can be discovered in it by any test, nor is there any fact which affords a presumption that it contains water; the supposition therefore that it does would be purely gratuitous, obviously advanced to support an hypothesis. But farther, dry ammoniacal gas is resolved by the action of electricity

for the water cannot be ascribed to the ammoniacal gas,

* The estimate by Gay-Lussac of the quantity of water in muriatic acid gas being equal to 1-4th of its weight is inferred from experiments, in which the product of the combination of the acid with the base is insoluble, and appears to have no affinity to water, as muriate of silver or of lead. It may be inferred, therefore, to retain little or none of the water of the acid, and hence the production of water to the amount of 1-5th or even 1-6th of the weight of the acid, in an experiment where the product must retain a portion of the water combined with it, is a near coincidence.

 into

into hidrogen and nitrogen gasses; there is no deposition of moisture, and there is no intermixture of oxigen, as there must be were the water decomposed by the electricity. If the water obtained in the preceding experiments were supposed to be derived from the ammonia, it must therefore be maintained without any proof; it is contrary to all probability, that these gasses, which have scarcely any sensible attraction to water, should be capable of holding in solution the large portion indicated by the experiment. And if recourse be had to the hypothesis of unknown quantities of water in gasses, and if all these assumptions are to be made without any proof, are there not much stronger reasons for admitting its existence in muriatic acid gas, the affinity of which to water is so strong? It is obvious however, that were assumptions so numerous and gratuitous to be admitted in defence of an hypothesis, no experiment in chemistry could be rendered conclusive. That the water obtained in this experiment can have no such origin is farther apparent from comparing the quantity of it with the quantity of ammonia. The specific gravity of ammoniacal gas is to that of muriatic acid gas as 60 to 124, or it is less than one half. In combining them about equal volumes were employed. Since the quantity of water obtained was equal to at least $\frac{1}{4}$ of the weight of the acid gas, it is equal of course to $\frac{1}{2}$ of the other. If that water then were supposed to be derived from the ammoniacal gas, and on Mr. Davy's hypothesis it would be necessary to suppose the whole of it derived from this source, we must suppose, that, after being dried, this gas contains nearly half its weight of water. Yet no portion of this can be discovered in it, nor even detected when it is resolved by decomposition into its elements, hidrogen and nitrogen gasses. To add any illustration to this would be superfluous.

of which it would amount to near half.

Farther experiments to be communicated.

The statement of some additional experiments on this subject, and of a few experiments likewise on some of the compounds, as Mr. Davy regards them, of the oximuriatic principle with metallic bases, I must, from the length of this, reserve for another communication.—I am, with much respect, Your most obedient servant,

JOHN MURRAY.

P. S.

P. S. In stating in my last letter, that the result I had observed, of dry oximuriatic acid gas not acting on carbonic oxide gas, was confirmed by the very same result having been obtained by Gay-Lussac and Thenard, I ought to have added, that it had also been obtained by Mr. Davy. In his account of "a combination of oximuriatic gas and oxigen gas," he states, among other properties of oximuriatic gas prepared in its pure state, that "it does not act on nitrous gas, or muriatic acid, or carbonic oxide, or sulphureous gasses, when they have been carefully dried*." That Mr. Davy does not state this on the authority of others is evident, not only from the manner in which the sentence is expressed, but also from this, that he is giving an account of the properties of this gas in its state of purity, in which state there was no certainty of its having been obtained in former experiments, as chemists were not aware, that it might have an intermixture of oxigen, by which its properties and chemical agencies are materially modified. He gives this as a property of the *pure* gas, and of course he would not have done so without having ascertained it.

Statement in a former letter confirmed by Mr. Davy.

VIII.

Analytical Formulæ for the Tangent, Cotangent, &c. In a Letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

HAVING frequently noticed in your valuable Journal remarks and discussions on mathematical subjects, I have been induced to send you one or two theorems, the investigation of which has afforded me some degree of amusement; under the impression, that they may not be quite uninteresting to that class of your numerous readers, who have not made very profound advances in analytical science. They are, as far as my reading goes, new, and lead to the summation of some series, hitherto, I believe, unattempted.

Analytical formulæ for the tangent, cotangent, &c.

* Phil. Trans. for 1811, p. 156; or Journal, vol. XXIX, p. 269.

1. Cotan.

1. $\text{Cotan. } A = \frac{1}{A} - \frac{2\,A}{\pi^2}\left\{\frac{1}{1^2 - \alpha^2} + \frac{1}{2^2 - \alpha^2} + \frac{1}{3^2 - \alpha^2} + \text{\&c.}\right\}$, where $\pi = 180°$, and $\alpha = \frac{A}{\pi}$.

For, by the common trigonometrical formula,

$$\text{Sin. } A = A\left(1 - \frac{\alpha^2}{1^2}\right)\left(1 - \frac{\alpha^2}{2^2}\right)\left(1 - \frac{\alpha^2}{3^2}\right) \times \text{\&c.} \quad \therefore \text{hyp. log.}$$

$\text{sin. } A = \text{hyp. log. } A + \text{hyp. log. } \left(1 - \frac{\alpha^2}{1^2}\right) + \text{hyp. log. } \left(1 - \frac{\alpha^2}{2^2}\right) + \text{\&c.}$: therefore taking the differentials,

$$\frac{\text{cos. } A}{\text{sin. } A} = \text{cotan. } A = \frac{1}{A} - \frac{2\,\alpha\, d\alpha}{d\,A} \times \left\{\frac{1}{1^2 - \alpha^2} + \frac{1}{2^2 - \alpha^2} + \text{\&c.}\right\}$$

$$= \frac{1}{A} - \frac{2\,A}{\pi^2} \times \left\{\frac{1}{1^2 - \alpha^2} + \frac{1}{2^2 - \alpha^2} + \text{\&c.}\right\} \quad (1)$$

In the same way we may deduce the second theorem I propose to offer.

$$2.\ \text{Tan. } A = \frac{2\,A}{\left(\frac{\pi}{2}\right)^2} + \left\{\frac{1}{1^2 - \beta^2} + \frac{1}{3^2 - \beta^2} + \frac{1}{5^2 - \beta^2} + \text{\&c.}\right\};$$

where $\beta = \frac{2\,A}{\pi} = 2\,\alpha$. For, since $\text{cos. } A = \left(1 - \frac{\beta^2}{1^2}\right)\left(1 - \frac{\beta^2}{3^2}\right)$ $\left(1 - \frac{\beta^2}{5^2}\right)$. &c. $\text{hyp. log. cos. } A = \text{hyp. log. } \left(1 - \frac{\beta^2}{1^2}\right) + \text{hyp. log. } \left(1 - \frac{\beta^2}{3^2}\right) + \text{hyp. log. } \left(1 - \frac{\beta^2}{5^2}\right) + \text{\&c.}$ ∴ differencing again as before,

$$-\text{tan. } A = \frac{-\text{sin. } A}{\text{cos. } A} = -\frac{2\,\beta\, d\beta}{d\,A}\left\{\frac{1}{1^2 - \beta^2} + \frac{1}{3^2 - \beta^2} + \frac{1}{5^2 - \beta^2} + \text{\&c.}\right\},$$

and therefore $\text{tan. } A = \frac{2\,A}{\left(\frac{\pi}{2}\right)^2}, \left\{\frac{1}{1^2 - \beta^2} + \frac{1}{3^2 - \beta^2} + \frac{1}{5^2 - \beta^2} + \text{\&c.}\right\}$ (2)

By the combination of the two formulæ, for sin. A, and cos. A, we may obtain an elegant analytical expression for tan. A.

$$3.\ \text{Tan. } A = \frac{\text{sin. } A}{\text{cos. } A} = \frac{A\left(1 - \frac{\beta^2}{2^2}\right)\left(1 - \frac{\beta^2}{4^2}\right)\left(1 - \frac{\beta^2}{6^2}\right)\text{\&c.}}{\left(1 - \frac{\beta^2}{1^2}\right)\left(1 - \frac{\beta^2}{3^2}\right)\left(1 - \frac{\beta^2}{5^2}\right)\text{\&c.}}$$

$= A \times \frac{1^2.\ 3^2.\ 5^2 \ldots \text{ to infinity}}{2^2.\ 4^2.\ 6^2 \ldots \text{ to infinity}} \times \left(\frac{2^2-\beta^2}{1^2-\beta^2}\right) \times \left(\frac{4^2-\beta^2}{3^2-\beta^2}\right) \times \left(\frac{6^2-\beta^2}{5^2-\beta^2}\right) \times$ &c. Now from this we derive, by taking as before the logarithmic differentials on both sides, $\frac{d \tan. A}{d A \tan. A} = \frac{2}{\sin. 2 A} =$

$$\frac{1}{A} + \frac{2\beta\, d\beta}{d A} \times \left\{ \frac{\frac{-(1^2-\beta^2)+(2^2-\beta^2)}{(1^2-\beta^2)^2}}{\left(\frac{2^2-\beta^2}{1^2-\beta^2}\right)} + \frac{\frac{-(3^2-\beta^2)+(4^2-\beta^2)}{(3^2-\beta^2)^2}}{\left(\frac{4^2-\beta^2}{3^2-\beta^2}\right)} + \frac{\frac{-(5^2-\beta^2)+(6^2-\beta^2)}{(5^2-\beta^2)^2}}{\left(\frac{6^2-\beta^2}{5^2-\beta^2}\right)} + \&c. \right\} = \frac{1}{A} + \frac{2A}{\left(\frac{\pi}{2}\right)^2} \times$$

$$\left\{ \frac{3}{(1^2-\beta^2)(2^2-\beta^2)} + \frac{7}{(3^2-\beta^2)(4^2-\beta^2)} + \frac{11}{(5^2-\beta^2)(6^2-\beta^2)} + \&c. \right\}$$

If in this form for A we write $\frac{A}{2}$, and divide by 2, it becomes $\frac{1}{\sin. A}$

$$= \frac{1}{A} + \frac{A}{2\left(\frac{\pi}{2}\right)^2} \times \left\{ \frac{3}{\left(1^2 - \left(\frac{A}{\pi}\right)^2\right)\left(2^2 - \left(\frac{A}{\pi}\right)^2\right)} + \frac{7}{\left(3^2 - \left(\frac{A}{\pi}\right)^2\right)\left(4^2 - \left(\frac{A}{\pi}\right)^2\right)} + \&c. \right\} \qquad (3)$$

which is the third formula I designed to demonstrate.

If in (1) for a^2 we write a, and transpose &c., we obtain $\frac{1}{1^2-a} + \frac{1}{2^2-a} + \frac{1}{3^2-a} + \&c. = \left(\frac{1}{A} - \cot. A\right) \times \frac{\pi^2}{2A}$; or, writing for A, $\pi\sqrt{a}$, $\frac{1}{1^2-a} + \frac{1}{2^2-a} + \frac{1}{3^2-a} + \&c. = \frac{1}{2a} - \frac{1}{2\sqrt{a}} \times \cot. \pi\sqrt{a}$ $\boxed{1}$

In (2) for β^2 write a, and for A, its value $\frac{\pi}{2}\sqrt{a}$; it becomes $\frac{1}{1^2-a} + \frac{1}{3^2-a} + \frac{1}{5^2-a} + \&c. = \frac{\pi}{4\sqrt{a}} \times \tan. \frac{\pi}{2}\sqrt{a}$ $\boxed{2}$

By multiplying

Analytical formulæ for the tangent, cotangent, &c.

By multiplying this last equation by 2, and subtracting $\boxed{1}$ from it, we obtain

$$\frac{1}{1^2-a}-\frac{1}{2^2-a}+\frac{1}{3^2-a}-\frac{1}{4^2-a}+\&c.=\frac{\pi}{2\sqrt{a}}\left\{\cot.\ \pi\sqrt{a}+\tan.\ \frac{\pi}{2}\sqrt{a}\right\}-\frac{1}{2a}=\frac{\pi}{2\sqrt{a}}\times\frac{1}{\sin.\ \pi\sqrt{a}}-\frac{1}{2a} \qquad \boxed{3}$$

In (3) for $\left(\frac{A}{\pi}\right)^2$ write a; and, transposing $\frac{1}{A}$, multiply by $\frac{\pi^2}{2\,A}$; and for A write its value $\pi\sqrt{a}$ we get

$$\frac{3}{(1^2-a)\ (2^2-a)}+\frac{7}{(3^2-a)\ (4^2-a)}+\frac{11}{(5^2-a)\ (6^2-a)}+\&c.=\frac{\pi}{2\sqrt{a}\times\sin.\ \pi\sqrt{a}}-\frac{1}{2a}$$ which is identical with the value of $\boxed{4}$

$$\frac{1}{1^2-a}-\frac{1}{2^2-a}+\frac{1}{3^2-a}-\frac{1}{4^2-a}+\&c.$$ found before.

Should you think these trifles worthy of insertion in your excellent publication, they are entirely at your disposal; and at some future time it is not impossible, that I may again intrude myself on the notice of your readers through the same medium.

I remain, Sir,

Your most obedient humble servant,

A LOVER OF THE MODERN ANALYSIS.

IX.

IX.

Meteorological Results: by James Clarke, *M. D., &c., late Physician to the Nottingham General Hospital, &c., and now resident Physician at Sidmouth.*

To Mr. NICHOLSON.

SIR,

THROUGH the medium of your Philosophical Journal I have for the last four years published an annual meteorological table, deduced from a Journal, which I kept at Nottingham; but being obliged, in consequence of serious indisposition, to change my residence to this place, the chain of observation is unfortunately broken. I send you however a table, which contains the result for the first six months complete, as taken at Nottingham; and for the last four months of the year at Sidmouth. Two months are necessarily lost. As this place has been gradually and deservedly rising into favour as a retreat for consumptive and debilitated invalides, a regular and accurate account of the weather becomes a matter of much interest to the public. Impressed with this opinion I waited only for the arrival of my barometer, &c., from Nottingham, to commence my observations upon the same plan that I had hitherto followed, and which my residence here in the practice of my profession will enable me to continue.

Meteorological observations at Nottingham,

and at Sidmouth.

The latter to be continued.

Sidmouth, as its name imports, is situate on the banks of the Sid, a very small river which here enters the sea; the town is built in a beautiful vale bounded on both sides by long lofty hills, which form its eastern and western sides; and toward the north it is screened by Gittisham and Honiton hills; but it is completely open to the south, where the sea forms a pretty little bay, bounded by Salcombe hill on the east, and Peak hill on the west. This is one of the small bays nearly in the middle of that large bay, which is bounded on the east by the Isle of Portland, and on the west by the Start Point. Thus protected, it is not surprising, that Sidmouth, among the places recommended on the

Situation of Sidmouth.

Its advantage to the invalids.

the southern coast for their sheltered and salubrious situation, should hold preeminence. It is entirely free from fog, and stands unrivalled for the clearness of its atmosphere, circumstances certainly well worth the serious attention of the invalide. The hedges of Devonshire are large and rich, and Sidmouth is closely surrounded with them: the walks and rides in the vicinity are thus sheltered from the burning sun, or the cold wintry winds.—"In the vernal and autumnal parts of the year the numerous lanes, which intersect and divide this rich valley, are truly delightful; the country then seems one universal garden*."

General remarks on the observations.

The barometer, of the portable kind, made by Jones of Holborn, is fixed to a standard wall; the observation is made daily about two o'clock; and at the same time the height of the thermometer is taken. At this time the barometer is supposed to be at the medium for the twenty four hours, and the thermometer at the maximum. As the temperature is considered to be at the lowest about an hour before sunrise, it would be impossible to keep a correct account, without the use of a register thermometer; the instrument employed for this purpose is of Six's construction. It is necessary to attend particularly to this circumstance, as observations made at eleven o'clock at night (a very common time) will not hold a just comparison with these, by which you ascertain the lowest degree to which the thermometer has fallen, since the last observation was made; without a little reflection on this subject, a very incorrect opinion might be formed of the temperature of this place.

I am, sir,

Your obedient servant,

JAMES CLARKE.

Sidmouth, Devon,
January the 13th, 1812.

* See The Beauties of Sidmouth, 12mo, sold by Longman, &c.

Meteorological

Meteorological Table for Nottingham and Sidmouth.

1811.	Thermometer				Barometer				Weather Days			Winds								Rain.
Months	Maximum	Minimum	Medium	Greateſt range in 24 hours	Maximum	Minimum	Medium	Greateſt range in 24 hours.	Fine	Fair	Wet	N.	N. E.	E.	S. E.	S.	S. W.	W.	N. W.	In inches and decimals
Nottingham.																				
January	51°	13°	30°	12°	30·52	29·06	29·81	·69	8	15	8	1	3	9	1	3	8	12	3	·97
February	52	22	37	11	30·08	28·85	29·40	·68	8	8	12	—	—	2	6	8	8	3	5	1·21
March	57	26	41	10	30·51	29·10	30·00	·89	19	6	6	1	6	8	5	3	6	9	4	1·47
April	70	22	46	11	30·10	29·06	29·66	·46	14	6	10	2	3	8	9	6	6	3	7	1·04
May	74	34	53	10	30·01	29·39	29·69	·51	4	11	16	1	6	7	9	7	9	1	5	3·73
June	76	37	55	9	30·18	29·44	29·83	·31	11	6	13	3	6	1	4	3	4	7	4	1·11
July																				
Sidmouth.																				
August									11	7	13									
September	73	36	55	12	30·38	29·22	30·16	·51	19	2	9	4	2	3	16	1	5	3	2	—
October	67	32	55	10	30·25	28·84	29·75	·73	10	4	17	2	—	2	3	2	13	2	1	—
November	60	26	45	10	30·51	29·40	30·06	·50	19	3	8	11	2	—	—	—	5	3	10	—
December	54	22	37	13	30·40	29·16	29·84	·60	16	2	3	12	3	—	—	—	6	4	8	—

X.

METEOROLOGICAL JOURNAL.

	Wind	Pressure. Max.	Min.	Med.	Temperature. Max.	Min.	Med.	Evap.	Rain	
12th Mo.										
Dec. 7	S W	29·56	29·35	29·455	54	52	53·0	—	—	☽
8	S	29·40	29·23	29·315	53	40	46·5	·10	·42	
9	S W	29·08	28·90	28·990	48	36	42·0	4	·50	
10	Var.	29·67	29·08	29·375	41	32	36·5	—	2	
11	N W	29·96	29·67	29·815	45	32	38·5	—	—	
12	S W	29·96	29·86	29·910	49	33	41·0	—	1	
13	W	29·85	29·77	29·810	54	35	44·5	·22	·30	
14	S W	30·00	29·85	29·925	42	30	36·0	—		
15	S W	30·00	29·50	29·750	47	35	41·0	—	·18	●
16	S W	29·58	29·39	29·485	42	36	39·0	—	1	
17	S W	29·86	29·58	29·720	40	31	35·5	—		
18	S W	29·86	29·75	29·805	52	33	42·5	·25	3	
19	S W	29·70	29·68	29·690	52	46	49·0	—	·13	
20	W	29·68	29·60	29·640	53	49	51·0	—	·16	
21	N W	30·15	29·60	29·875	49	27	38·0	—	3	
22	W	30·18	30·06	30·120	45	32	38·5	—		☾
23	W	30·15	30·06	30·105	51	38	44·5	—		
24	N W	30·19	30·15	30·170	43	28	35·5	—	5	
25	S E	30·20	29·98	30·090	39	24	31·5	—		
26	S W	29·98	29·55	29·765	32	21	26·5	—	—	
27	N E	29·27	29·16	29·215	34	26	30·0	—	·14	
28	N W	29·67	29·27	29·470	35	27	31·0	—	5	
29	N	29·96	29·67	29·815	32	22	27·0	—		○
30	S W	30·08	29·96	30·020	30	25	27·5	—		
31	S W	30·08	29·88	29·980	35	31	33·0	—		
1812										
1st Mo.										
Jan. 1	S W	29·88	29·70	29·790	43	34	38·5	—		
2	S	29·70	29·56	29·630	48	31	39·5	—		
3	S	29·55	29·46	29·505	44	29	36·5	—	3	
4	N	29·46	29·37	29·415	38	33	35·5	—	·41	
5	N W	29·76	29·47	29·615	37	29	33·0	·60	·26	
		30·20	28·90	29·708	54	21	38·06	1·21	2·73	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES.

NOTES.

Twelfth Month 7, 8. Much wind; showery. 9. The wind at 9 a. m. E., yet sounds came freely from the westward, together with the clouds. *Nimbi*: rainbow; showers through the day; a brilliant twilight. 10. A gale of wind a. m., then fair. 12. A dripping mist. 14. Clear day; an extensive redness on the twilight.

N. B. The regular chain of notes will now suffer a few weeks' interruption, by the removal of the author to London: the temperature, wind, and rain, are registered at Plaistow, as usual.

First Month 2, 1812. About 10 a. m. there having been no rain for a considerable time, a few light clouds, just formed, and coming from the westward, suddenly exhibited a segment of a rainbow, terminating above and below, at the edge of the mass of cloud. As the latter advanced by the north, and became denser, the arch increased, and became at length nearly complete; the eastern extremity descending towards the earth, with the usual appearance of rain under the clouds. The western end now began to fade, and was soon reduced to a pale white, which gradually pervading the whole, this pleasing phenomenon disappeared, having lasted about ten minutes. It afforded an example of rain, formed and propagated in the atmosphere with such rapidity, as scarcely to give time for the previous appearance of buoyant drops in the form of cloud. The observation was made about four miles N. of London.

RESULTS.

Prevailing winds westerly.

Barometer: highest observation 30·20 inches; lowest 28·90 inches; Mean of the period 29·708 inches.

Thermometer: highest observation 54°; lowest 21°; Mean of the period 38·06°.

Evaporation 1·21 inches. Rain 2·73 inches, including several products of snow.

L. HOWARD.

LONDON,
First Mo. 18, 1812.

XI.

XI.

Appendix to the Meteorological Journal, containing Observations on Rain and Rain Gauges.

Observations on rain and rain gauges.

IT is a fact long established, that two rain gauges, placed at different heights, afford unequal products; the lower commonly yielding more than the higher. The following table gives the results of observations on this subject made during twenty successive days, on which rain fell at Plaistow; the elevation or depression of the mean temperature and direction of the prevailing wind being added.

Table of the Products of Rain in the Gauges No. 1 and 2, with the Changes of wind and Temperature.

1811		Wind	Temp. +	Temp. —	Rain. No. 1	Rain. No. 2	REMARKS.
Tenth Mo. OCT.	24	Var.		5°	5	8	Misty rain about mid-day; little wind veering from S. W. to E.
	25	S		4	—	—	
	26	Var.	2°		·45	·50	Showers chiefly by night.
	27	S E	2		·10	·11	Rain by night.
	28	Var.		1	·44	·44	Clear a. m. with dew; *nimbi*; vane S. E. p. m a heavy shower to S.; wind veered by S to N. W.; then much cloud and rain.
	29	S W			·18	·18	Showers.
	30	Var.	2		8	·14	Three currents in the air—see Journal.
	31	W	3		·13	·14	Rain by night.
Eleventh Mo. NOV.	1	S W	6		5	·11	Much cloud with a fresh breeze.
	2	S W		2	6	·14	Cloudy; much wind; stormy night.
	3	S S W		4	6	8	Rain by night.
	4	W		2	—	—	
	5	S W		2	9	·25	Stormy a. m.; wet p. m.
	6	S W			·31	·50	Showery day; *cirrostratus* evening; wet night.
	7	N E		3	6	7	Rain by night.
	8	S	1		·16	·19	Cloudy; drizzling.
	9	Var.	3		·29	·34	
	10	S W		3	·19	·21	
	11	N W		3	1	3	Windy night; *nimbus* at sunset.
	12	N W	2		·11	·22	Windy night.
					2·82 in	3·73 in	

The

The upper gauge, No. 1, is fixed on the N. W. angle of a glass turret, or observatory, on the house top, having a small vane and a conducting rod a few feet to the S. and S. E., but no other commanding object near it. The whole of the amounts of rain given in the tables in the *Athenæum* during 1807, 1808, and part of 1809, were obtained with this gauge. The gauge No. 2, the products of which I now prefer to register, is placed on a grass plot, about 70 feet from the west front of the house. Their difference in elevation is about 43 feet. Observations on rain and rain gauges.

It appears, from the total result of these observations, that about one fourth of the rain which fell in twenty days was formed within 50 feet of the Earth's surface.

In attending to the manner in which the rains fell, the cause of the frequent difference in the products of the gauges, was, at times, obvious. When they were alike, the abundance and active appearance of the clouds in the higher atmosphere, together with the transparency of the lower, indicated that the whole supply might very well be derived from above. On the contrary, in several cases of excess in No. 2, the lower air was very turbid, showing that the decomposition of vapour was going on quite down to the surface of the Earth; or, in other words, that the raining clouds, though not distinguishable as aggregates to us, who were enveloped in them, actually swept, or rested upon that surface.

On the first day, when the products were 5··8, the mean temperature was lowered 5°, probably by the effect of the gentle easterly current, which decomposed the vapour near the surface. On the 28th of the tenth month, when the results were large and equal, a southerly current appeared to prevail in the region of the clouds, with, probably, a N. W. wind above it; by which the vapour coming from the south was decomposed. This was accomplished at a distance from the Earth, and the mean temperature was lowered 1°. These two cases may elucidate the phenomenon without a long train of reasoning.

If we admit, that a portion of the atmosphere, contiguous to the Earth's surface, may be so cooled by a superior portion moving in a different direction, or with different velocity

Observations on rain and rain gauges.

city in the same, as to become filled with a fine mist, which is ultimately resolved into clouds and rain, we shall perceive, that a set of rain gauges, placed at various heights within this portion, ought to collect less and less rain, as we ascend; since each stratum deposits its own redundant water, and transmits that of the higher ones.

But if the source of the rain be in a middle current, the lower part of which is above all the gauges, they ought all to afford like quantities; unless, indeed, the lower air be so dry, at the same time, as somewhat to lessen the bulk of each drop by evaporation; in which case, (as is said to have happened in some instances,) the products will be found *larger* as we ascend.

But there is another source of discordant results, which seems not to have been enough attended to. It exists in the deflection of the rain by accidental currents. On the 25th of the ninth month, finding in the gauge, No. 2, 0·46 of an inch, while No. 1 had only 0·12 of an inch, I suspected that the wind, which came in squalls from the W., had a share in producing the difference. I took, therefore, two other gauges, No. 3 and No. 4, and on the 27th, placed No. 3 in the gutter, near and on a level with the W. parapet of the house, and No. 4 about 20 feet in a line to leeward, at the same height, but sheltered between the roofs. It was then beginning to rain in moderately large drops; wind fresh at S. W. After two hours and a half, I found in No. 3 0·08, and in No. 4 0·11 of an inch; No. 1, on the ground, having also 0·11 of an inch. I removed No. 4 about 40 feet to leeward, near the E. parapet, and got in an hour and a quarter from No. 1 0·08, No. 2 0·15, No. 3 0·12, No. 4 0·14 of an inch. The rain continued six hours, with a steady wind, and was at times heavy: near twice as much fell on the ground gauge as on that at the turret; and the results of the other gauges proved, that some part of the difference must be attributed to the wind. It appears, that the stream of air, obstructed by the W. front of the house (which has a contiguous building fronting S.), and rising in a curve, carried with it a part of the rain over the windward guage, to let it fall on the leeward; for the latter had more than its due proportion, the former less.

Thus

Thus *rain may be drifted as well as snow*, and it will be very difficult to affix a gauge to any part of a building, so that its products shall not be affected by partial currents, diminishing or overcharging them; and allowance must doubtless be made in the results of the foregoing table for this source of errour. Observations on rain and rain gauges,

On the whole, as the proper subject of calculation and comparison is the rain on the surface of the ground, *this* is the proper ordinary situation for the gauge; and it should be as remote as possible from all objects that might give rise to eddies in the stream flowing over it. As a further defence, both from these and from sudden frosts, the bottle, into which the rain enters from the funnel, should be placed in a box, sunk in the ground; above which there should be a cavity sufficiently large to admit the funnel, with its mouth level with the ground, and a free space of a few inches round it, the whole being laid with turf, both to keep it neat and to break the spray in heavy showers. On a future occasion I purpose to give a description of the instrument I now use as a rain-gauge, and to explain the principles of its construction.

L. H.

Eleventh Month 27, 1811.

XII.

A Reply to some Observations and Conclusions in a Paper just published in the 2d volume of the Medico-Chirurgical Transactions "On the Nature of the Alkaline Matter contained in various Dropsical Fluids, and in the Serum of the Blood": By GEORGE PEARSON, *M. D. F. R. S., &c.*

To W. NICHOLSON, Esq.

SIR,

I WAS favoured a few weeks ago, by Dr. Marcet, the author, with the above named paper. In it I have the satisfaction to find many of the facts confirmed, and none contradicted, Facts found by the author confirmed,

dicted, which I have published in the Philosophical Transactions for 1809 and 1810* on expectorated matter and purulent fluids, except with regard to the alkaline impregnations. My experiments informed me, that expectorated matters, and pus, contain potash neutralized by an animal substance, or by an acid destructible by fire. I likewise found, as I prosecuted my inquiries, that there is the same kind of alkaline impregnation in the blood, in the dropsy fluids, in the fluid effused by vesicating with cantharides, in the fluid secreted from the nose owing to a catarrh, and even in the urine. And as I did not find the soda alkali in a similar state, I concluded, that hitherto this alkali had, probably, been mistaken for the potash. In the ingenious paper however, which has occasioned this reply, it is asserted, that the alkali in combination with the animal matter is the soda; but it is inferred, that potash is also present, not in the state I discovered, but united to muriatic acid.

except with respect to the alkali.

Neutralized potash found in various animal fluids.

This said to be soda,

by Dr. Marcet and Dr. Wollaston,

It would not be treating the public justly, if I did not say, that the paper before me contains an inquiry conducted conjointly by the writer, Dr. Marcet, and Dr. Wollaston; as Dr. Marcet represents, I allow, very fairly, to enhance the credit of his statement. Considering the power of these allied opponents, the odds are fearful. But confiding in the assurance of Lord Bacon, that induction by experiment equalizes the mental faculties among different men†, I shall, with this palladium, obey the summons to the *arena*—at the worst issue, with such adversaries it would be glorious to fall in the struggle:

.... Agimus, prô Jupiter !....
.... causam ; et mecum confertur Ulysses.

who merely give their own experiments.

To enable the chemical public to judge rightly of the different conclusions above declared concerning the kinds and states of the alkalis existing in the animal fluids, the evidence of the opposing parties must be heard. The adverse party however have not attempted to invalidate my

* See Journal, vol. XXV, p. 216, 260; and vol. XXX, p. 17, 113.

† Nostra vero inveniendi Scientias ea est ratio, ut non multum ingeniorum acumini et robori relinquatur, sed quæ ingenia et intellectus fere exæquat.—Bacon's Novum Organum, sect. I XI.

evidence

evidence by showing, that the conclusions are unjustifiable, but have merely exhibited their own experiments and conclusions. This mode of procedure, I apprehend, is not according to the laws of controversy; and it compels me to make a statement of, at least, some of the most decisive experiments for my conclusions, previously to the examination of the contravening evidence.

1. 961 grains of exsiccated *sputum* on incineration and fusion afforded 45 grains of saline substances, consisting of 35 grains of cubical crystals of muriate of soda, and the rest were spicula and uncrystallized salt, amounting to 10 grains. These 10 grains were separated for distinct examination. They manifested the properties of alkaline matter. On adding liquid tartaric acid to this alkaline matter, also liquified, an effervescence ensued, with a precipitate of supertartrate of potash only—"*certainly yielding no soda-tartrate of potash*"—with nitro-muriate of platina a grain or two of this saline matter produced a reddish precipitate

Experiments to show that it was potash. Exp. 1.

Now if muriate of potash and carbonate or subcarbonate of soda had existed, the result must have been soda-tartrate of potash and muriate of soda; or tartrate of potash and muriate of soda. This latter result is not so probable as the former on account of the very large proportion of alkali to any other possible salt. The quantities too were obviously sufficient for producing compound salts determinable by the eye unassisted with glasses.

2. By digesting 2500 grains of desiccated sputum in 4 pints of alcohol of spirit of wine, the clear tincture decanted from off the undissolved matter afforded on distillation 140 grains of resinlike substance, which manifested no alkaline properties, but it indicated slightly acidity. Exp. 2.

A portion of this resinlike substance, being mixed with liquid tartaric acid, was subjected to distillation, but neither muriatic nor any other acid was disengaged. This I conceive shows, that no muriate of potash existed.

25 grains of this matter were acted upon by successive affusions of nitric acid; and, on boiling to dryness and ignition, the deflagration which took place produced a charcoal-like mass, containing potash. Hence this alkali had been united to something destructible by fire.

According to computation the 140 grains of resinlike matter contained 28 grains of potash united to matter destructible by fire, and 18 grains of muriate of soda, with an inappreciable quantity of ammonia and phosphoric acid, beside the animal matter.

The matter undissolved by alcohol in this process afforded by incineration and fusion a mass, consisting of 23 grains of muriate of soda, with a very small proportion of potash, mixed with 23 grains of phosphate of lime, traces of magnesia, iron, and a sulphate, also a minute portion of utterly indissoluble vitrified matter. If potash had existed in union with muriatic acid, it must have appeared in the fused mass left undissolved after digestion in alcohol; but potash did appear in a naked state after ignition and fusion of the matter dissolved in alcohol.

Exp. 3. 3. By digesting 4000 grains of sputum in two pints of rectified spirit of wine, the same results were obtained, excepting that the resinlike matter contained a much larger proportion of muriate of soda and animal matter.

Exp. 4. 4. Twenty ounces of ropy sputum, by digestion in ten pints of distilled acetic acid, afforded, on evaporation of the clear liquid separated from the coagulated matter, a soft extract. This extract deliquesced partially on exposure for a few days to the air, but it manifested no properties of alkali. By exsiccation, ignition, and fusion, of a little of this deliquesced matter, it afforded an aqueous solution, which precipitated abundantly supertartrate of potash on adding tartaric acid; and a reddish precipitate fell on the addition of platina solution.

The whole of this extract, being exsiccated, was digested in rectified spirit of wine, affording a blackish tincture. After evaporation to dryness, it became liquid by 24 hours exposure to the air. It was almost entirely acetate of potash. I believe acetate of soda neither dissolves in alcohol, nor deliquesces, but, independently of these properties, the alkali united was proved to be potash.

Examination of the evidence adduced against this conclusion.

I shall call no other evidence from a great mass, which remains in my published papers. If I were to follow the example of my adversaries, I should also not trouble myself to examine their evidence; but, as the question cannot be decided

decided without such an examination, I beg permission to perform this duty.

1. *Of the fluid of the spina bifida.*

In the ten printed pages of experiments on this fluid by Dr. Marcet, I can only perceive, that there is evidence for the existence of an alkaline subcarbonate; yet it is said, "Soda may be inferred from the effervescence with acids." The alkaline matter was treated with alcohol, and thus it was separated from the muriate; the alcoholic solution, being decanted and evaporated to dryness, a residue, "*supposed to consist of acetate of soda,*" was obtained, which weighed between 17 and 18 per cent. of the mass. "*Oximuriate of platina produced no precipitate.*"—I remark, 1st, That the first result only shows the presence of charcoal acid.—2d, The acetate of soda is not, I believe, dissoluble in alcohol; but it is well known, that acetate of potash is so; however, if there be the authority of experiment for the dissolubility of acetate of soda in this menstruum, still the experiment is equivocal. It was easy for the adverse party to have decided this question by the test of tartaric acid, provided there was an adequate quantity of matter for the trial.—3dly, I remark, that, there being no precipitation with the platina solution seems to me to prove nothing, as the whole quantity of matter treated could not reasonably be supposed to amount to more than a small fraction of a grain, too small for the detection of potash by means of platina solution, or even probably by the more sensible test, tartaric acid, which was not used. Yet the ingenious writer has not only inserted *soda* among the impregnating ingredients of the fluid under examination, but also boldly denoted the proportion to the centesimal part of a grain. I shall, in another part of this communication, I believe, demonstrate, that this analysis does not warrant the statement of the composition of this dropsical fluid given in such precise terms; for, on the ground of cogent analogy, I cannot doubt that one or more ingredients are present, but not inquired for by experiment, nor enumerated. Hence not only is the analysis objectionable with respect to the ingredients, but the proportions. It is true, that in a subsequent

Experiments on the fluid of the spina bifida.

Remarks on these.

sequent part of the investigation, the deficiency seems to have been perceived and acknowledged; but, if so, it will not be an easy task to justify the publication of perhaps an inaccurate analytical statement in opposition to my experiments, which have not been refuted.

2. *Of the fluid of hydrocephalus internus.*

Experiments on the fluid of hydrocephalus internus.

A few grains of the saline matter of this fluid consisted of cubic crystals, mixed with spicular and opaque globules. The assertion is several times made, that the spicular crystals and opaque globules were carbonate of soda, and that most of the cubes were muriate of soda, but some of the smaller ones were found to be muriate of potash. The proofs for the assertion are from the two reagents I employed in the same inquiry, *namely*, tartaric acid, and platina solution, for the potash; and "the carbonate of soda was identified not only by tests indicative of the absence of potash, but also by its forming rhomboidal instead of prismatic crystals when treated with nitric acid." Now I apprehend our judges will deem this evidence unsatisfactory, and that much more decisive proofs will be reasonably expected. I beg permission to ask, whether or not the laborious experiments upon a large scale, which I instituted to exhibit evidence of the exclusive existence of the potash alkali, are to be disproved by the rhomboidal figure of the crystals in place of prismatic, seen perhaps only by a magnifying glass, and in the quantity of a grain or two dispersed over a comparatively extensive surface; and whether or not the absence of potash, indicated by tests operating upon minute quantities, is unequivocal evidence, and ought to counterpoise experiments with quantities affording products of which no doubt can be entertained. I do not question the accuracy, but I hope it is proper to take a farther objection against the competency of the experiment asserted for the presence of soda, and absence of potash. On the most important point, which occurred in the inquiry, the kind of alkali existing in the fluids, I do conceive, that more experiments, and particularly detailed, are necessary to effect the disproof of what I have published, and to command assent that soda, and not potash, is present. Is it satisfactory

Remarks on these.

has been thought right, however, to assume an hypothesis, or more truly two hypotheses, to account for the potash in the menstruum of alcohol: viz. 1, to imagine, that muriate of potash is present; 2, that it is dissoluble in alcohol. If potash was present in the indissoluble residue, it was most important to have exhibited the state in which it existed. It was not difficult to determine, if doubted, the state of the potash in the alcohol, by burning the residue left on evaporation, which would have denuded it if united to the acetic acid, but not if united to muriatic acid. Supposing, however, it be judged right, to receive these experiments as evidence of the facts asserted by the adverse party, I beg to claim the right also of opposing the contravening evidence above delivered in stating the results of a similar experiment. From this representation I submit to our judges, whether or not I am entitled to object to the enumeration of subcarbonate or carbonate of soda as one of the impregnating ingredients of serum, and especially to the proportion denoted in centesimal parts of a grain, in a mass amounting to 7 or 8 grains, consisting of 7 different substances.

Having communicated merely the information of the senses through the intermede of experiments*, it will be determined by the chemical world, whether or not the opposing party have demonstrated errours in observation, or illegitimate conclusions. I am of opinion, that the best founded conclusions are but provisional; and of course, that chemistry has not yet attained the rank of a science, or at least, of a demonstrative science. This opinion seems just from a retrospective view of the varying states of chemistry for the last hundred years. Many of the theories of the illustrious Stahl were, for half a century, admitted as demonstrations of the agency of phlogiston. That these doctrines were erroneous was evinced by the succeeding discovery of the agency of oxigen, especially manifested by the ever-to-be-lamented Lavoisier; and the pneumatic doctrine, in some parts, has

Chemical conclusions at present but provisional.

* Sensus enim per se res infirma est, et aberrans; neque organa ad amplicandos sensus aut acuendos multum valent, sed omnis verior interpretatio naturæ conficitur per instantias, et experimenta idonea et apposita; ubi sensus de experimento tantum, experimentum de natura et re ipsa judicet.—Bacon's Novum Organum.

lately

lately been rendered doubtful, if not exploded, by the woudrous achievements of professor Davy. Considering this progressive state, I offer the conclusions, that potash, and not soda, is the alkali existing united to animal matter in the animal fluids I examined, merely as provisional. That potash does also exist in them united to muriatic acid is not inconsistent with my experiments; but the experiments of my learned friends do not appear to authorise such an inference. The discovery however will be partly due to them, if hereafter the fact be substantiated.

Microscopic chemistry.

I cannot close this communication, until I shall have said a few words concerning the high encomiums on what is called *microscopic chemistry*, accompanied by the bitter philippic against the "dismal, large, subterraneous laboratory". Chemistry must now, we are told, be transferred "to the comfortable fireside of the drawing room"—from Vulcan's foul stithy to my lady's chamber. This *elegant* change is to give "new impulses" to the advancement of the science; and new schools are to arise under new auspices. Most happy shall I be to find these eutopian prospects realized. It seems however more than probable, that the successful impulses already given by the chemical schools of my very learned and approved good masters, Cullen, Black, and Fordyce, will retain the principal cultivators in the paths now opened. And with regard to the scene for operations, the privilege of *taste* will be asserted; for that is indeed not disputable either in chemistry, or elsewhere—Becher's *taste* was opposite to that of the ingenious new advocates, "nec quicquam præ carbonibus, venenis, fuligine, follibus, et furnis valere potest"—*Phys. Subter. Præf.* The Lord High Chancellor of England not long ago declared in court, that he would not pay "sixpence" for the rapturous notes of Mara, or Catalani.—This also was a matter of *taste*, and no one disputed it; it was only observed by a large majority, that his Lordship had "no music in his soul, and was not charmed by concord of sweet sounds"—no more. The value of a tree is best known by its fruits; and accordingly, to inform the judgment of the public by practical examples, and as some return for the notice with which my papers have been honoured, I shall, with your permission, offer

offer for your next number a few remarks on the publication in general, which has produced this communication; in which, whatever differing opinions may subsist, I assuredly must admire the ingenuity, and respect the knowledge of my honourable antagonists.

G. P.

George Street, Hanover Square,
January 14, 1812.

XIII.

On the Culture and superior Colouring Qualities of Madder raised by Mr. WILLIAM SALISBURY, *of the Botanic Gardens at Sloane Street and Brompton, from Seeds presented to the Society of Arts, &c. by* J. SPENCER SMITH, *L. L. D., who procured them from Smyrna**.

SIR,

I Herewith send you two samples of extract of madder, one of which, marked A, is produced from the root of the Smyrna kind, a plant which I have not heard of being before introduced into this kingdom, the seeds of which I received from you, and which you informed me had been procured at the request of the Society of Arts &c. from Smyrna, by J. Spencer Smith, Esq. I sowed the seeds in my Botanic Garden, at Cadogan place, in April 1808, in a soil rather inclining to clay; and I have the satisfaction to find, from this experiment, that there is every appearance of its being cultivated with considerable success; for, if I might venture to state a calculation made of the crop from the small quantity grown, the produce would be upwards of fifteen hundred weight of the fresh root per acre. Madder seeds from Smyrna sown. Their product.

The above estimate is made on the supposition, that the seeds were sown in drills at one foot distant from each other, which appears to me to be the best mode for its cultivation, I am thus particular, as I conceive I shall be doing my The seeds should be drilled at a foot distance.

* Trans. of the Society of Arts, Vol. XXVII, p. 104. Samples of the seed received from Mr. J. S. Smith are preserved in the Society's Repository, also the coloured liquors referred to by Mr. Salisbury.

country

country a service, if it will induce any person to attempt the culture of this madder on a larger scale. I beg leave to observe, that the first attention which I paid to this valuable vegetable, after I had raised it from the seed, was to ascertain satisfactorily whether the superior quality of its colouring matter depended on the plant itself, or if it was merely owing to climate, or other local substances; which often occasion a great difference in the quality and value of many other productions of a similar nature. To prove this I had extracts made in the same manner with the prepared Dutch madder of our shops, which did not bear any comparison in point of colour with that of mine; but fearing, that the Dutch madder might be damaged by the mixture of some extraneous substance, I made a similar extract from the fresh roots of the common rubia tinctorum, which had for some years past been growing in my garden at Brompton, and the extract marked B is the result, and is much inferior in colour to that from the Smyrna seed: though the extracts were both obtained in the same way, viz. by boiling the roots and making a precipitate from them by alum and vegetable alkali.

Superiority of the colouring matter.

I flatter myself I have here been instrumental in the introduction of a plant, producing a very valuable dye, and hope we may not be long under the necessity of depending upon a foreign market. If any gentleman would wish to make experiments relative to its growth, or if any seeds of a similar nature should come into the Society's possession, I shall be happy to make experiments with them, having appropriated a piece of ground in my new botanic garden solely for such purposes. I must confess, that I have great pleasure in the above communication, as it will prove, that benefits occur from botanical institutions; and that the opinion formed by some persons, that the study of botany is a dry nomenclature, is founded in errour; for certainly much good will arise from botanical investigations to medicine, the arts, and manufactures.

Ground in Mr. Salisbury's garden appropriated to experiments.

Study of botany advantageous.

I am, with great respect, Sir,

Your obedient and humble servant,

WILLIAM SALISBURY.

Brompton, April 26, 1809.

DEAR

DEAR SIR,

In answer to your farther inquiries respecting the madder procured from the Smyrna seed, I beg leave to observe, that, with regard to the management of the seed, I found it to succeed extremely well in drills in the open ground. I also tried some in a hot-bed, which also succeeded perfectly well; but the old seed, some of which I had from you this spring, will not grow. I consider it to be a variety of the common rubia tinctorum, but of a more robust growth, and superior in colouring matter. These plants thrive exceedingly in my new botanic garden in Sloane street, and I flatter myself, that I have been instrumental in introducing an article, which gives to cotton the most beautiful and permanent red colour in existence. *Management of the seed.*

Many former attempts to cultivate madder in England have failed, I understand, on account of the calico-printers formerly requiring it in a powdery state; but since the establishment in this kingdom of the Adrianople or Turkey red dye upon cotton, some thousand tuns in weight of madder roots from the Levant are annually used in Great Britain for dyeing that colour, for which use this kind of madder in the fresh root will be found superior. *Use of the root in calico printing.*

I am informed, that, by the application of the Society of Arts, &c., to Government, madder roots grown in England are exempted from tithes. *Madder roots tithe free.*

I have every reason to believe, that for use in painting much finer colours than the present may be obtained from the root of this plant by spirituous or acetous extracts; but I forbear at present farther experiments, in order to increase as much as possible my remaining stock of plants; and this appears necessary, as I find the seed I had left will not vegetate this spring, and I apprehend, that such seed as may now remain in the Society's possession will be useless. *Their use for pigments.* *Old seed will not vegetate.*

I shall therefore proceed to increase my present stock of the plants from offsets and cuttings of the roots. If the above account is found deserving of the society's attention, it is much at their service, and they shall be welcome to some of the roots, when I have farther propagated them. They blossomed abundantly, but did not produce seed, a circumstance which *The plants blossomed, but did not seed.*

which I observed also in the common kind growing near it: I have therefore endeavoured to increase it by other means, which may be done to any extent, but being now particularly engaged, the means I employ must be a subject of future communication.

I am, with great respect,
Dear Sir,
Yours very truly.
WILLIAM SALISBURY.

Botanic Garden, Brompton,
Nov, 16, 1809.

XIV.

Note on the Analysis of Hyalite: by Mr. BUCHOLZ*.

Analysis of hyalite.

HAVING experienced a loss of 8 per cent in my analysis of the hyalite, published in Geblen's Journal for 1806, vol. I, p. 202, and not knowing to what to ascribe it, I was much pleased at receiving in the autumn of 1807 a sufficient quantity, to verify my former experiments. Suspecting that this loss was owing to water, I put 75 grs of hyalite, broken into small pieces, in a Hessian crucible, and kept them at a white heat for half an hour. The fragments became muddy and friable, and had lost 4·75 grs. As I have every reason to believe, that this loss is owing entirely to water, it follows, that 100 parts of hyalite give

Component parts.

Silex	92.
Water	6·3
Some flocks of alumine and loss	1·6
	100.

Hydrates of silex.

Hyalite then approaches to the noble opal, which contains 0·1 of water, according to Klaproth; and still nearer to the common opal, which contains 0·05. According to the same chemist, all these stones must be true hydrates of silex. The specific gravity of this stone, taken by Mr. Kopp, is 8·15 [most probably a misprint for 2·15. C.]

Specific gravity.

* Ann. de Chim. vol LXXIII, p. 328.

SCIENTIFIC

SCIENTIFIC NEWS.

Wernerian Society.

AT the meeting of this Society on the 30th of November, prof. Jameson read a paper on granite. Three principal granite formations, and two of sienite, were described. Two of the granite formations are primitive: the third, transition: and of the sienites, one is primitive, and the other transition. He described particularly the appearances, that present themselves at the junctions and alternations of the granite and sienite with gneiss and *killas*, (which last is probably a newer gneiss), and the relations of these rocks to mica-slate, clay-slate, gray-wacke, and gray-wacke-slate. The descriptions were illustrated by numerous sections and specimens from Galloway, island of Arran, and other parts of Scotland.—The professor likewise read an account of the natural history of a new genus of concamerated fossil shell. In describing this shell, he employed the usual zoological language; but in detailing the other particulars, the method followed was that used in giving the natural history of minerals. Paper on granite. New genus of fossil shells.

At the same meeting the secretary read a communication from the Rev. Mr. Fleming of Flisk, containing an account of a bed of fossil shells, which occurs on the banks of the Frith of Forth, near Borrowstounness. The bed is three feet thick, nearly three miles in extent, and lies about 33 feet above the present level of spring tides. The kinds of shells which compose this extensive bed, are still found in a recent state in the Frith.—At the same meeting, also, Mr. Leach read a description of a new British species of echinus, which he observed in plenty at Bantry Bay in Ireland, and which he proposed to call e. lithophagus, as it forms a small hollow for itself in the substance of the submarine rocks. Bed of fossil shells. Echinus lithophagus, a new species.

At the meeting of this society on the 14th of Dec., professor Jameson read a short general account of the geognosy of the stewartry of Kirkcudbright. It would appear from the Geognosy of Kirkcudbright.

the professor's description, that the greater portion of this part of Scotland is composed of gray-wacke, gray-wacke slate, and transition-slate, with subordinate beds of *transition-porphyry*, transition greenstone, and flinty slate. But three tracts, the first of which contains the mountain of Criffle, the second Cairnsmuir of Dee, &c., and the third Loch Doune, are composed of granite, sienite, sienitic porphyry, and killas. The sienite and granite in some places are covered by the killas; in other places the granite and sienite rest upon the killas; and professor Jameson also observed the killas alternating with beds of granite and sienite, and veins shooting from the granite into the adjacent killas. The granitous rocks, beside felspar, quartz, mica, and hornblende, also contain imbedded rutilite, titanitic iron-ore, and molybdena; and, in rolled masses of a reddish-coloured sienite, crystals and grains of zircon were observed. Prof. Jameson also stated several of the characters of the killas, described the magnetic pyrites it contains, noticed its affinity with certain rocks of the transition class, and exhibited specimens to illustrate this affinity.

Temperature of the Gulf Stream.

At the same meeting there was read a series of thermometrical observations on the temperature of the Gulf Stream, by Dr. Manson, of New Galloway: and a description of a new craniometer, proposed by Mr. W. E. Leach, illustrated by a sketch.

Craniometer.

A fossil powder analogous to resins.

A potter, at Dijon, has found between some strata of fossil wood in the territory of Louhans a fossil vegetable powder. It is of a cinnamon colour, burns with flame, and emits a peculiar smell approaching to that of olibanum. Like *amber* and mineral caoutchouc it appears analogous to resins.

Musical lectures.

Dr. Crotch will commence his Course of Lectures on Music at the Surry Institution, on Tuesday the 4th of February; and will continue them on each succeeding Tuesday evening, until completed.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1812.

ARTICLE I.

On the different Sorts of Wood, with some Remarks on the Work of Du Thouars. *In a Letter from Mrs.* Agnes Ibbetson.

To Mr. NICHOLSON.

SIR,

I Know not whether the account I have given of the mechanism of Botany has convinced my readers, or done justice to my subject; the latter is indeed impossible. I shall however (though thoroughly sensible of my deficiencies) renew the discussion as soon as the spring bestows a fresh return of her treasures for dissection. In the mean time I shall give a farther idea respecting the formation of *wood*, in some measure answering to the new Treatise on Vegetation, received from France, and written by Aubert du Thouars, celebrated for his studies in physiology, and who appears (like myself) to have taken nature for his guide, and left books for a future consideration; thinking it of greater consequence to ascertain a few absolute facts, than

On the treatise of Aubert du Thouars.

than to collect a confused mass of uncertain details, that lead to no end.

The bud another kind of seed.

Du Thouars agrees with me, that each bud is only another kind of seed, having its cotyledons the same, and wood vessels reaching from the bud to the root, and ending in a radicle, so that each tree may be esteemed (in this respect) a collection of plants, really distinct from each other, though joined under a double cover. But in my opinion he carries this idea much too far in giving the same appellation to both root and wood, for they certainly greatly differ in many respects, which he seems not to have noticed, beside the former having a double vessel, till it joins the radicle, while the wood vessel is single; as I have before described in my former account of this substance. He is also of my opinion respecting the supposed circulation of sap, which he appears persuaded does not exist, the liquor flowing to the terminating branches only, to give life to fresh matter, shooting from every extremity where the flow of sap will allow it to form; indeed, we are now so well acquainted with the different parts of the stem, that, if there were any returning vessels, they could not (I think) but be known. But he is undoubtedly mistaken in saying, that each leaf has a wood vessel, as well as the bud; for, if this was true, the stem must be as wide again. I have traced this part with such exact attention, as not easily to be mistaken; and have always found, that the set of wood vessels, after meandering all over the leaf, returns on the upper side of the stalk, and thus enters another leaf, that the same collection may serve many hundreds. It is not difficult to follow them, and is most convincing.

But the root and wood differ in many respects.

No returning vessels.

Each leaf has not a separate wood vessel.

Where the flower bud is formed.

There is nothing that gives me more astonishment than the blindness of physiologists respecting the formation of the flower bud. Du Thouars, as well as Mr. K., thinks it is formed in the new wood, next the bark, and believes the bud is generated by the sudden mixture of the wood and bark, as if they did not run side by side throughout the plant; a strange mistake! I confess it is this discovery "of the shooting of the bud" I am most proud of, first because it is the foundation of many important facts, that lead to consequences of no little moment; secondly, because it is

so very plain, so easily seen, that, strip off the bark, and a child would acknowledge and understand it; as the buds appear coming out from the interior of the wood in every part. But how many truths does this substantiate! It proves which is the vital part of a plant; that the impregnating line, which runs into the seed, is likewise found running next the pith in the stem; that this is the line of life, from which also all buds proceed; that the seed and bud are the same thing, at least differing in trifles only; and that they both owe their existence to this same vital part, one shooting in the flower, the other in the stem of a plant. In herbaceous plants this same line runs within the pith, but equally gives life to bud and seed; hence the truth of Linnæus's observation, when he gave such consequence to the pith of some plants.

Consequences deduced from this.

There is something so curious in the manner in which the bud is first united with the sap vessel that nourishes it, that, as I have not exactly shown the process, I shall give it here. I have described the manner in which the line of life first generates the bud by forming a knot on that line, and breaking the outward ends. Each end then becomes a bud, and it is the business of the wood to form a covered way for the passage of the bud to the exterior, which it does by bending some and raising other parts of its vessels, in the middle of which the bud passes to its cradle; but no sooner does the knot form on the line, than it becomes a signal for the root to send up a vessel for the nourishment of the new bud, and by the time it reaches its cradle, this vessel (loaded with sap) arrives at the same place, and fastens itself to the bud, affording it that nourishment the milky juice of the albumen could no longer give it. It is exactly the same process as that in the seed, where the milky albumen first supports, and is then succeeded by the nourishing vessels.

Manner in which the bud is first united with the sap-vessel that nourishes it.

I shall now give a curious proof, that the wood is the only part which carries the sap for the nourishment of the plant; and that the plant dies, if the wood vessel is lost. In tracing the various diseases of plants, especially in our kitchen vegetables, it occurred to me to examine thoroughly that which is called smut in potatoes; and compare it with the

The wood the only part that conveys the sap for nourishing the plant.

Diseases of plants.

the disorders which are found in the brocoli and turnip. In the brocoli the first apparent symptoms are a shrivelling and drooping in the stem of the plant; it for some time languishes like a consumptive patient, and then dies after a long illness. Curious to find what could be the cause of this disorder, and its effects on the interior of the plant, and why the plant ceased to gain that nourishment, which the earth seemed so fitted to give; I resolved to dissect the plant with care. To prepare myself for thoroughly understanding it, I took a brocoli plant growing in other ground, and perfectly healthy, dissecting and drawing it, as I give it at fig. 1, plate V. It will be seen how many radicles it has, how many wood vessels, both in the root and stem. I now laid open the diseased plant with the greatest care: but how excessive was my surprise to find, that almost all the wood vessels had disappeared, though the plant was twice the size of the healthy brocoli, and of a bulbous form in the root, see fig. 2; that in place of the wood vessels, I could see only tubercles filled with water, and that, instead of near a hundred radicles, two solitary ones, with their appropriate wood vessels, were all the plant had to bring it support. The bulbs of water were composed of a loose matter something like the pith of trees, but very large, and without one ligneous particle; in short the complaint appears like a dropsy in the plant. From various trials it was plain to be seen, that the defect arose from the earth; since if pease, beans, vetches, &c. were substituted for the brocoli, turnip, or potato, they would grow admirably, but the same disorder would equally attach to those vegetables just named, if placed in that earth. The cause then was the failure of suitable nourishment for those plants in that ground; and its consequences were the decay of the parts intended to bring that sort of sap, which the ground was not able to bestow. This shows also, that there is a great variety in the sap, though we are not able to discover it; indeed in our trials so much of the most important and delicate essence may evaporate, during the first attempt to ascertain the ingredients, that we cannot thoroughly trust to our knowing all that enters into the composition of this astonishing mixture. I cannot conceive that any thing but decay

Disorder of the brocoli.

The cause in the earth and not in the plant.

Ingredients of the sap not known.

M

decay could cause the disappearance of the wood vessels*; and am more confirmed in this idea, as I have always found that it is the constant consequence of the stoppage of the sap, whenever it takes place during the life of the plant. This I before showed, when I endeavoured to prove how mistaken physiologists were in supposing that part of the stem of the tree was void of sap, and was only filled in the newest part.

On summing up the evidence I should conclude, that the ground is too cold and moist for these vegetables. On dissecting turnips and potatoes growing in the same ground, I find it exactly the same disorder as that in the brocoli, that is a total disappearance of the ligneous parts, both in stem and root. The tubercles of water mentioned in the brocoli, turnip, &c. are always full of stinking water, as the putridity is just beginning. May not the disorder of these plants in some measure serve to prove, with the rest of the facts already adduced in my former letters, that the sap is conveyed by the wood alone for the nourishment of the plant? I think the body of evidence I have at different times given now nearly amounts to certainty, that the sap runs in the wood alone, and does not circulate. This disorder must not be mistaken for that called the scab in potatoes, which is a malady that proceeds from a cryptogamian plant, first growing on the outside of the potato, in which insects fix and lay their eggs, to insure food to their hatching young; which soon dive into the interior, and cover the root with blotches.

The ground too cold and moist.

Sap conveyed by the wood alone.

Scab in potatoes.

The next part which belongs to the wood, and which I wish thoroughly to explain, is that which appertains to the balls. I have shown, that there are in plants two sorts, one which generally regulates the mechanism of plants, (the account of which I gave in my last letter on mechanical botany); the other the ball found in the wood of trees. When a bud, formed of the knot of the line of life, and passing through its covered way to the exterior, is by any accident severed from that line, the bud stops, and can proceed no farther, though the wood vessel joins to it. In time the albumen, which surrounds it, changes to wood, and the bud, though its interior never grows, yet continues

Ball found in the wood of trees.

to

to increase in wood, as long as the hardened parts about it will permit, adding one little row of wood each year, but this soon ceases. It is perfectly detached from the rest of the plant; and afterward pushed towards the exterior by the growing part of the plant, while the sap continues to circulate round it and within it. The constant pressure these balls receive makes them grow to an inconceivable hardness; and, when taken out, I have found them from twelve inches to a quarter of an inch in diameter, and so regularly formed, (see fig. 5) that had I not taken them myself from the tree, I should have been persuaded they were just turned in a lathe. Some are round, but with a single wood vessel attached to them; some formed like a spinning top. Here is an old tree, that has formerly been in a hedge-row, that has three balls about twelve inches in diameter. Carpenters, when they find them, use them as heads, cogs of wheels, or for any purpose that requires extreme hardness.

Balls in the wood.

The forming of hungry wood.

The next peculiarity of the wood I have never yet thoroughly explained is the exact reverse, in effect, of the ball; and the cause of both can be described by a drawing: it is what the French call hungry wood, it proceeds from some accident, a severe season, lightning, or injury the tree has received. Some wood is much more liable to it than others. It is a formation that is quickly finished, but that stops many of the sap vessels, so that the wood is soft and poor. I have often found a piece in the middle of beautiful perfect wood so diseased. Hungry branches are often seen shooting from the roots of trees. The rose, the viburnum, the barberry, and many shrubs as well as trees, are subject to this defect. Among trees the plane, the ash, and the lime in particular are most liable to it; it is to be known by its difference of appearance. To show it I shall draw a piece of solid good oak, and a piece so affected, having these specimens now in my possession. Fig. 3 is the healthy piece, fig. 4 the diseased oak: *c c* are the sap vessels, *d d* are the intermediate parts of both. When young, the wood is wider between the sap vessels, but not near so far apart as the diseased wood, which never appears to contract, as all perfect wood does. It is this effect, that has made many physiologists think, that the sap vessels dried up, when the wood

No wood without sap.

wood became old; but it only leaves the intermediate parts, which constantly contract. The sap vessels are rather enlarged than lessened by age; and grow vastly firmer by the strong support they thus gain by reduction.

Trifling difference in the albumen.

I shall now turn to the wood of annual or herbaceous plants: it is formed of two sorts, those which like the shrub have only a narrow piece of ligneous matter; and those the wood of which is formed in compartments, something like the manner of the leaf stalks, that is, round vessels of wood surrounded by albumen, which altogether appear infinitely larger than the common vessels so denominated, the spiral wire being enclosed within the wood. These vessels are set like spots in the circular pith; the number of sap vessels increases, if the plants last a long season, as well as the rows in the herbaceous vegetables, if fine weather prolong their existence: this I have before hinted at, in a former letter, Vol. XXVIII, p. 249. It would seem that this albumen, as well as that which surrounds the ligneous vessels in the leaf, never became wood; for let the vegetables be ever so old, it still retains the clear and unformed appearance it has at the first moment; and let it be where it will, it is always to be known. It is this we call nourishing vessels in the leaf. I think it should be called by the name of clear albumen, to distinguish it from that which afterward becomes wood. I have examined both; and, as far as the eye can judge, they appear perfectly the same; but in taste the clear albumen, which is that found in the seed, the leaf stalk, and herbaceous plants, is bitter, which the other is not, the other albumen is the new row of wood in trees and the foundation of the seed, with the first formation of the embryo. In all these the sap vessels afterward shoot. When I tried both by decomposition the residuum was the same; indeed the matter I can get for trial is so little, it is hardly a fair chemical process.

I should have continued to give an account of the ideas of Aubert du Thouars, could I have thoroughly understood the rest of his opinions; but he is not so intelligible as de Mirbel; and (if I may be allowed to say so) has rather too much given way to system and imagination. I should suppose it is necessary to pass from step to step in this study, perfectly distinguishing

distinguishing first the separate parts of a plant, and then the different sorts of vegetables, and keep to this; for if the seed is not exactly divided from the leaf, the stem from the root, and so on to the rest; and if the tree is not distinguished from the shrub, and the semiplant, and herbaceous plant, from the cryptogamia; it must inevitably create confusion, as they differ so essentially in form and manner of growing. It is also most desirable to give but one part at a time explaining this with a print, that may make it understood, or the most perspicuous writer, on so dark a subject, will want elucidation. But this gentleman, (though he began so well) has launched into some system, which he seems to hold out, I think, as some mathematical demonstration in the vegetable world. I regret that he has not divided it from his physiological labours, as both might have profited by this arrangement. I have however so much to apologize for myself, that I am the last person, that should criticise others.

Rest of du Thouars work.

I am, Sir,
Your obliged servant,
AGNES IBBETSON.

II.

On the Action of Elastic Fluids on dead Animal Flesh: by Mr. HILDEBRANDT.*

The putrescency of bodies must be much influenced by the surrounding gasses.

CONVINCED, that experiments and observations on the spontaneous analysis of organic substances must be very instructive, and tend to illustrate their nature, I have always endeavoured to impress on the hearers of my chemical lectures, how great must be the influence of the elastic fluids, that surround putrescent bodies, either in accelerating or retarding this natural process. This also has determined me, to investigate the subject with great care; and I venture to lay before the public the results, as constituting a series of experiments, that may tend to the advancement of the science.

Experiments on the subject.

* Ann. de Chim. vol. LXXIII, p. 200. Translated from Gehlen's Journal, vol. VII, No. 2.

It

It will be proper to premise:

1st, That I always employed the same kind of flesh, namely beef, in order to be more certain, that the differences observed arose only from the action of the different elastic fluids: that the flesh used at one time was taken not only from the same animal, but from the same muscle; that it contained no fat, but muscular fibre alone; and that the pieces were of equal size, cut in parallelopipedons, and proportional to the vessels: The substance employed.

2dly, That I took only the flesh of an animal that had been dead two hours:

3dly, That I used only the last portions of gas evolved, in order that it might not be mixed with any of the air in the receivers; and that I employed the gasses soon after they had been prepared. The atmospheric air I took from a very airy garden. The gasses.

4thly, The vessels were placed in a room, into which the sun never entered. The windows also, facing the north, were very small, to avoid the action of light, which I purpose to examine on a future occasion. The temperature of this room is cool in summer; and in winter high enough to keep water from freezing. If, however, there were any reason to be apprehensive of frost in the night, I removed the vessels into my lecture-room, which joined it. Place of the experiments. Temperature.

I took three modes of enclosing the flesh in the gasses; and, to avoid repetition, I shall denote them by the following expressions: 1st, over water: 2d, over mercury: 3d, in an empty bottle. The gas employed in three modes.

1. For the experiments over water, cylindrical jars were taken, containing from 92 to 98 cubic inches, Paris measure, and filled over a pneumatic trough. Pieces of meat 3½ inches long, 1 inch broad, and ½ of an inch thick, were then introduced into them. These were suspended from two cross pieces of brass, having a point ½ an inch long to fix the meat on, and supported by a stem of the same metal, with two other cross pieces for a foot. When this stand, with the meat thus disposed, had been introduced through the water into the jar, a plate was passed into the trough, and the jar placed upon it, so that it might be removed from the trough, and set on a table. When the water rose in the Manner of conducting the experiments over water:

the jar, which is the case when the temperature diminishes, or the gas is absorbed, care was taken to add water, in order to prevent the introduction of atmospheric air. In this way the contact of atmospheric air is avoided, but it is attended with the inconvenience of wetting the whole of the surface of the meat, and of the gas and meat being in contact with the water in the jar.

in a bottle stopped with a cork:

2. For the experiments in an empty vessel, that is to say, containing neither water nor mercury, bottles resembling those for wine were taken, but with wider mouths. These were filled with gas over the pneumatic trough, and let stand till they were well drained. The meat was then introduced, keeping the mouth of the bottle above the water, corked as quickly as possible, and the cork well luted with paper and glue, or the neck of the bottle placed in water. On turning up the bottle, the piece of meat falls to the bottom, to which it commonly adheres in consequence of its moisture. By this mode we avoid wetting the meat; and neither the meat nor the gas is in contact with so large a quantity of aqueous vapour: but it has this inconvenience, the meat has been in contact with atmospheric air, and a little is always introduced with it, when the cork is put into the bottle. This process cannot be employed with nitrous gas.

and over mercury.

3. For the experiments over mercury, small jars, of 3 or 4 cubic inches only, were filled with the gas to be subjected to experiment; and pieces of meat, an inch long, ½ an inch broad, and 2 lines thick, were then passed through the mercury with the fingers, and introduced into the jars. By this process the contact of atmospheric air is avoided; as well as of aqueous vapour, when the gasses have been procured over mercury. But these experiments I could execute only on a small scale; as I had not a sufficient quantity of mercury for any thing more.

A little water, or mercury, should always be left in the jars, in order that the air, when expanding, may not get out of them. In the experiments over mercury it affords the advantage of preventing the vessel from being overturned.

First

First series of experiments, over water, begun the 25th of March, and ended the 4th of April, 1808.

1st set of experiments over water.

Temperature.

The temperature of the air out of doors was always between 8° of Reaum. [50° F.] in the shade, as for instance on the 30th of March in the morning, and 5° R. [43·25° F.], as on the 25th of March at noon.

1. *Oxigen gas.*

Exp. 1. With oxigen gas from nitrate of potash.

The gas was obtained from nitrate of potash.

1st day. March the 25th. The meat is become evidently redder, and even of a finer red than in nitrous gas.

2d and 3d days. The red colour has diminished, but the meat has still a fresh appearance.

4th and 5th days. The same.

6th and 7th days. The redness has gradually diminished; and the meat is moister than that in nitrous or in hidrogen gas.

8th day. The meat grows damp; it begins to get livid; and little hemispherical and almost transparent drops appear standing separate from each other on its surface.

9th day. The drops become more numerous, and grow gradually opake and whitish; so that the meat looks as if covered with small pox.

10th and 11th days. Putrefaction makes evident progress, the meat grows flabby, the moisture increases, and the surface dissolves; yet the drops may still be distinguished at the surface of the liquid, that covers the meat entirely.

On the evening of the 11th day the meat was taken out, after the jar had been replaced in the trough. It emitted a putrid smell, somewhat alliaceous, having some resemblance to that of phosphuretted hidrogen gas.

The meat continued to putrefy in the open air, as if it had been constantly exposed to it.

The oxigen gas was not much diminished in bulk. It no longer set fire to a candle, but a candle still burned in it for a moment with a little brilliancy.

2. *Hidrogen gas.*

Exp. 2. With hidrogen gas

This was prepared by dissolving zinc in sulphuric acid diluted with water.

from zinc and sulphuric acid. 1st day. March the 25th. In the course of a few hours the meat had acquired a dirtier colour; and at length became of a blackish brown, like meat that has been smoked after having been salted without the addition of nitre.

From the 2d to the 11th day. No change in the external appearance of the meat; particularly it has grown neither flabby nor moist: its firmness appears even to have increased; and it seems more hard and dry. It has lost its redness more and more, and is become browner than meat exposed to carbonic acid.

On the evening of the 11th day the meat was taken out. It had no sign of putrescency, and not the least offensive smell: the most that could be said was, that it smelt slightly sour. On exposure to the air it did not putrefy, but became dry: a few small white specks of mouldiness, however, were perceivable on its surface.

On turning up the jar, and applying a lighted candle to it, the gas took fire.

3. *Carbonic acid gas.*

Exp. 3. With carbonic acid expelled from chalk by nitric acid. This was prepared by dissolving chalk in nitric acid.

1st day. March the 25th. In the course of a few hours the meat had assumed a dirty colour, and afterward became brown; but it remained of a lighter colour than that in hidrogen gas.

From the 2d to the 11th day. At first it appeared to grow more livid and flabby, but after a few days no change was observed. The surface did not appear to get moist. The carbonic acid gas, used in the experiment, is absorbed by the water; and it even appears to be more readily absorbed than when pure, so that fresh gas must be added every day, to prevent the water from touching the meat.

On the evening of the 11th day the meat was taken out. It had no marks of putrescency; resembled meat that had been dressed; was flexible, without being moist or viscous; and had a slight acidulous smell, nearly like that of yeast turning sour. On exposure to the air it did not grow putrid, but dried, and its surface became covered with little white specks.

4. *Nitrous*

4. *Nitrous gas.*

Procured from nitric acid by means of copper, and received over water. Exp. 4. With nitrous gas from nitric acid and copper.

1st. day. March the 25th. The meat became of a much finer red than in atmospheric air; and for the first few hours it could not be distinguished from that in oxigen gas.

2d and 3d days. No change.

4th, 5th, and 6th days. The fine red colour diminished a little, but it still remained very lively.

From the 7th to the 11th day. No change was observable, except that the meat appeared to grow a little moist; but it did not liquefy at all at the surface, and its firmness even seemed to increase.

On the evening of the 11th day the meat was taken out. It had a fine red colour, was firm, and had no smell, not even of nitrous gas. It lost its redness when exposed to the air, in the course of a few hours; became brown; and dried much quicker than the meat of the two preceding experiments. Its surface did not become covered with white specks.

The gas, exposed to the test of oxigen gas, produced a quantity of red vapour, diminished greatly in bulk, and appeared not to differ perceptibly from common nitrous gas.

Second series of experiments, from the 5th of April to the 10th of June.

Second series of experiments.

The temperature of the air out of doors was 2·5° [37·75° F.] on the morning of the 18th of April, and 23·3° [84·4° F.] on the 17th of May at noon. Temperature.

That of the room was between 7° and 20° [47·75° and 77° F.].

The meat employed in the following experiments was of a paler colour, and appeared to have come from a younger animal. Meat used.

5. *Oxigen from red oxide of mercury. Experiment over mercury.*

Exp. 5. Oxigen from red oxide of mercury, over mercury.

1st day. April the 5th. The meat became redder.

2d and 3 days. No perceptible change.

4th

4th and 5th days. The colour grew paler.

From the 6th to the 8th day. The colour was destroyed, and the meat had the appearance of having been washed.

9th day. The drops appeared at the surface as in the first experiment.

18th day. The drops, became opake, appeared like the eruption of small pox. The meat preserved its firmness without liquefying, though the temperature was higher than in the former experiments.

From the 19th to the 21st day, May the 25th. Visible signs of putrescency were observed on the surface, the little drops ran into each other, and the surface became blackish.

An accident having overturned the jar, the gas escaped, and diffused such a strong stench throughout the house, that we were obliged to perfume it strongly, to get rid of the smell.

6. *Oxigen from nitrate of potash, in an empty bottle, stopped with a cork.*

Exp. 6. Oxigen from nitrate of potash in a corked bottle.

1st, 2d, and 3d day. The meat did not grow redder.

4th day. It grew pale.

5th to the 51st. No little drops were observed, but the meat gradully grew pale, became putrid, and liquefied at the surface. At length a considerable quantity of liquid, of a bad colour, was formed, and flowed down into the neck of the bottle.

The meat was covered with moisture. Its smell was not so strong as that of the preceding piece putrefied in oxigen gas, and of a different kind.

7. *Atmospheric air, over mercury.*

Exp. 7. Atmospheric air over mercury.

1st and 2d days. April the 5th and 6th. No remarkable change.

4th day. The meat is become very pale; paler than in the oxigen gas.

5th to the 51st. The little drops of liquid were not perceived. From the 8th day it became covered with moisture, and liquefied at the surface, but less than that in oxigen gas; and at the close it appeared less black than in pure oxigen

oxigen gas. On taking it out of the receiver, its stench was not so powerful, and it appeared redder when cut.

8. *Pure hidrogen gas from the vapour of water passed over redhot iron. Over mercury.*

1st day. April the 5th. The meat became of a crimson red. Exp. 8. Hidrogen from water by redhot iron, over mercury.

From the 2d to the 51st. No change was observed, except that the meat became a little brown; but it did not acquire a livid hue. It is remarkable, that this meat remained reddish, and preserved an appearance of freshness, while pieces in oxigen gas, and in atmospheric air, grew pale. When taken out of the jar, it had no smell. The gas examined at the end of the experiment rendered lime-water turbid.

9. *Pure hidrogen gas in a corked bottle.*

1st to the 51st day. The meat scarcely grew brown at all, but preserved its colour, only appearing a little moist. When taken out the 51st day it had no bad smell, but smelt like smoked meat. Exp. 9. Hidrogen gas in a corked bottle.

The gas, examined by the test of nitrous gas, gave no sensible diminution; it rendered lime-water a little turbid, and afterward burned vividly.

10. *Pure carbonic acid, from the calcination of chalk. Over mercury.*

1st day. The meat became crimsoned, as in hidrogen. Exp. 10. Carbonic acid, expelled from chalk by heat, over mercury.

2d to the 11th. No sensible change, and the meat looks very fresh.

13th to the 22d day. It grew paler.

51st day. The meat was uniformly pale, looked as if it had been dressed, and appeared nearly of the same firmness.

It was neither moist nor viscous; and had not the least smell, or any other sign of putridity.

The gas was absorbed by lime, except a trifling residuum, that did not amount to 0·01.

11. If this experiment be repeated in corked bottles, and the meat be enclosed in one while the gas is hot, and in the other not till it has grown cold; it will be found, that the meat Exp. 11. The former repeated in corked bottles.

meat in the cold gas will have kept well till the 16th day, but will have acquired an unpleasant smell; while that in the hot gas stinks on the 30th day, and is completely spoiled on the 60th.

12. *Nitrous gas, over mercury.*

Exp 12. Nitrous gas over mercury.

1st day. The meat became redder.

51st day. The meat has retained its fine colour, and is firm. The liquid, that has flowed from it, has assumed a fine red colour, and deposited a small portion of white matter, resemblimg fat, though the meat contained none.

67th day. June the 10th. The meat has retained its fine colour still, on which account I did not take it out, that I might see how long a time was necessary to effect its decomposition.

Third series.

Third series.

The temperature was the same as in the preceding series.

13. *Oxigen, over water, the jar contained only* 28·5 *cubic inches.*

Exp. 13. Oxigen gas over water.

1st day. The meat became of a fine red.

2d, 3d, and 4th. The meat retained its colour, and did not appear to putrefy.

6th day. Little transparent drops were observed; which increased in number and size on the 7th day, and grew turbid and red on the 8th.

9th day. The putrefaction was evident over all the surface, which began to liquefy. The gas diminished greatly in bulk. There is no doubt but the increase of temperature is the cause of the speedy putrefaction.

10th day. The gas, measured in the gasometer, had diminished 7 cubic inches. Being placed in contact with milk of lime, it diminished 6 5 cubic inches more. Supposing, that the 7 inches absorbed by water were carbonic acid, 13·5 inches of oxigen were expended, which must have formed 18·75 inches of carbonic acid gas*.

* The carbonic acid gas, as appears from the best recent experiments, must have been precisely the same in bulk as the oxigen gas expended. C.

The

The 15 inches remaining having been tested with nitrous gas, I found that they contained 5·4 of nitrogen, and 9·6 of oxigen. Thus the 28·5 inches of oxigen gas have been employed in

13·5 carbonic acid,
5·4 nitrogen,
9·6 oxigen.

14. *Atmospheric air.*

The meat putrefied, and was decomposed on the 48th day. The water rose considerably, and absorbed 21 cubic inches out of the 96, that the jar contained. The experiment having been deranged, I could not continue my observations. Exp. 14. Atmospheric air, over water.

15. *Pure hidrogen gas.*

1st day. The meat became of a poppy colour. Exp. 15. Hidrogen gas.

4th day. No change, except that the meat appeared dried.

6th day. Some mouldiness observed, that increased on the 7th day.

From the 8th to the 41st day no farther change was observed, except that about the 20th the mouldiness had disappeared. The meat resembled beef salted without nitre and smoked. It had not the least bad smell.

The gas, tested with lime-water, did not render it turbid. It burned with force and energy.

From these experiments it appears how necessary it is, to repeat them separately, in order to obtain some certain results. It appears however, that we may draw from them the following conclusions.

1. That hidrogen preserves, and even increases the firmness of meat, by drying it. That oxigen on the contrary diminishes this firmness, rendering the meat flaccid and moist. It is remarkable, that hidrogen preserves this firmness even over water, when the gas is saturated with moisture. General deductions.

2. That meat is changed and liquefied much more speedily in oxigen, when it contains nitrogen, as in atmos-

 pheric

pheric air, and in the gas from nitrate of potash, than when the gas is pure.

3. That nitrous gas resists putrefaction most powerfully; next to which comes hidrogen, and then carbonic acid.

4. That meat does not change so soon in oxigen gas, as in atmospheric air; but, when putrefaction has commenced, it proceeds with more energy than in atmospheric air, and diffuses a much more offensive smell.

5. That the colour of meat gets browner in hidrogen, and lighter in oxigen and in nitrogen.

6. That neither hidrogen, nitrous gas, nor carbonic acid, appears to undergo any sensible alteration from the meat included in it.

7. That oxigen gas, whether pure, or mixed with nitrogen, is converted into carbonic acid.

8. That part of the oxigen gas retains its properties, as in other combustions.

9. That, during the putrefaction of the meat in oxigen gas, nitrogen is produced; which nitrogen must be evolved from the meat, or oxigen has been converted into nitrogen.

10. When meat begins to grow putrid in hidrogen, it appears, that carbonic acid is evolved; but this does not take place, as long as the meat keeps without spoiling.

11. That on meat in oxigen gas little drops of water are formed, which resemble the eruption of small pox.

Continuation of the inquiry.

A continuation of my inquiries will be instituted for the purpose of verifying the facts I have announced; and particularly of ascertaining, whether the carbonic acid gas, found mixed with the hidrogen, existed in the meat; and of investigating the influence of light, and the luminous properties of stinking meat.

III.

Continuation of Mr. Hildebrand's *Paper on the Action of Gasses on dead Animal Flesh.*

Meat left a long time in nitrous gas.

I. I Have said in my 12th experiment, that, finding the meat not altered after having remained 57 days in nitrous gas,

gas, I resolved to leave it there a longer time. Accordingly I did not take it out till the 25th of August, when it had remained in contact with the gas for 134 days. In the first months the temperature was between 7° and 20° [47·75° and 77° F.], and in the last months it was between 12° and 23° [59° and 83·75° F.] The temperature therefore was much higher, than was necessary to favour the decomposition of the meat, yet it retained a fine red and fresh colour. The liquor however, which had such a fine red colour, lost this in some measure. Having taken the meat out of the nitrous gas, to examine it more carefully, I observed, that, wherever it had touched the sides of the glass, it had become yellowish. (This I presume was owing to the contact of the glass diminishing the action of the gas.) In other respects, it was still of a fine red, had a good degree of firmness, and did not stink in the least; but it had a slight smell of nitric acid, with which a little of a peculiar smell was observed. Thus we see, that a longer time and a higher temperature produced changes, which did not take place in a shorter time and at a lower temperature. Temperature, Results.

The white sediment, mentioned in my first experiment, was found on examination to be coagulated fibrin. When agitated in water, it exhibited itself in the form of the little strings, that remain after the washing of the coagulum of blood. Boiling water did not dissolve, but hardened it. Fibrin deposited.

The nitrous gas, that had been used for the experiment, when brought into contact with atmospheric air produced much red vapour; and the diminution of bulk was as great, as at the moment of its preparation. The gas apparently unaltered.

II. Having remarked in experiments 8 and 9, that the meat, which had remained 51 days in hidrogen gas at a temperature from 7° to 20° [47·75° to 77° F.], rendered lime-water turbid, I instituted the two following experiments. Carbonic acid produced in hidrogen gas.

16. *Hidrogen gas.*

I prepared this gas with zinc and dilute sulphuric acid, to obviate the objection, that the carbonic acid gas, contained in the hidrogen prepared by passing the vapour of water over a red-hot iron, might have originated from carburet of iron. Exp. 16. Hidrogen gas from zinc and sulphuric acid, over mercury.

 I en-

Temperature. I enclosed the meat over mercury. The thermometer out of doors, from the 23d of July to the 14th of September, was between 8·5° and 26° [51° and 90·5° F.]; that in the room between 11° and 20° [56·75° and 77° F.]. The shutters of the room were closed, except when the meat was examined.

Results. 1st day. July the 26th. In a few minutes the meat became of a brown and livid hue.

2d day. The colour became again a little red.

22d day. August the 17th. It retained a pretty red and fresh appearance, without any mark of putridity.

34th day. Every thing was in the same state, and appeared to remain so till the 54th day.

State of the meat the 54th day. Having taken the meat out on the 54th day, I found it as firm, as if it had been fresh; but it emitted an insupportable stench, differing however from that of meat putrefied in oxigen gas, or atmospheric air. Thus it appears, that organic matter can undergo a kind of alteration, and diffuse volatile principles occasioning noisome smells, without losing its cohesion, as in putrefaction properly so called.

Part of the gas converted into carbonic acid. The bulk of the gas in this experiment was diminished only according to the variation of the temperature. At 14° [63·5° F.] it was 4·75 cubic inches, French measure. When I passed it through lime-water, it rendered it very turbid; I therefore agitated it with milk of lime, and, having measured it afresh, it was reduced to 3 cubic inches, so that 1·75 cubic inch of carbonic acid had been formed.

17. *Hidrogen gas.*

Exp. 17. Hidrogen gas, over water. I placed a piece of meat in some of the same hidrogen gas; and the apparatus was kept with the preceding, and under precisely the same circumstances; except that the jar was over water, and contained 52 cubic inches.

Results. 1st day. July the 26th. The meat experienced the same change, as in the preceding experiment.

2d day. The colour appeared a little redder than in the experiment over mercury.

22d day. The meat looked red and fresh, and seemed drier than in the experiment over mercury. It appeared wrinkled.

The

The water has risen considerably in the jar, and rises every day.

54th day. The meat was of a fine red, but no longer wrinkled: it appeared drier, and resembled smoked meat. Taken out of the gas, it was as hard as smoked meat, but it diffused a horrible smell. State of the meat on the 54th day.

As the temperature was 20° [77° F.] when the experiment began, and was now but 14° [63·5° F.], the quantity of 52 cub. in. should be reduced, according to Gay-Lussac, 0·14; and, according to Schmidt, 0·17: but there remained only 36 cub. in. of gas; consequently 15·83, or 15·86, were absorbed. Diminution of the gas.

The gas, placed in contact with lime-water, and with milk of lime, diminished only a quarter of a cubic inch more; so that the total absorption was 16·08, or 16·11.

If the water and the milk of lime did not absorb as much gas in this experiment, as the milk of lime did after the experiment over mercury (for the ratio of 4·75 : 1·75 would give for the 52 cub. in., $19\frac{3}{19}$), it is nevertheless evident, that in both the experiments there was a considerable formation of carbonic acid, amounting to more than a fourth of the bulk of the hidrogen employed. A considerable portion of carbonic acid formed,

Though distilled water may contain carbonic acid, we cannot surely ascribe to it this great quantity of gas; particularly as it was continually rising in the vessel, so that it was absorbing the gas, and not giving it out. Besides, the experiment over mercury removes every doubt. The formation of carbonic acid therefore was owing to the decomposition of the meat: for hidrogen alone would remain for years over water or mercury, without being decomposed in any way. But though the gas, that remained after the separation of the carbonic acid, was hidrogen; it is equally certain, that part of the hidrogen had disappeared: for, had not this been the case, the bulk of the gas should not have been altered in the 17th experiment, and in the 16th it should have been considerably increased. by the decomposition of the meat; and part of the hidrogen lost,

As it is not probable, that this hidrogen combined with the meat in decomposition; since it appears, that meat gives out an hidrogenous vapour when it changes; it would follow, that the hidrogen, which disappeared, was combined apparently combined with or forming the carbonic acid.

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ed in the mixture of carbonic acid gas, which I cannot explain.

IV.

On the Nonexistence of Sugar in the Blood of Persons labouring under Diabetes Mellitus. In a Letter to ALEXANDER MARCET, *M. D., F. R. S. from* WILLIAM HYDE WOLLASTON, *M. D., Sec. R. S.**

MY DEAR SIR,

Nonexistence of sugar in diabetic blood.

IN reply to your inquiry respecting my experiments upon the nonexistence of sugar in the serum of diabetic persons, which I have mentioned to you at different periods, I am really ashamed to reflect how long I have suffered them to remain neglected, when I consider their tendency to elucidate a curious point of physiological research.

Attempt to detect it.

My first endeavours to detect sugar in the serum of the blood were made soon after perusing the second edition of Dr. Rollo's Treatise on the diabetes (which was published in 1798,) at the request of Dr. Baillie, who was so obliging as to furnish me with various specimens of diabetic blood and serum for this purpose.

Farther experiments.

The other set of experiments which I made with reference to the same question were not thought of till the following year. The inquiry was then left unfinished, and I never resumed it; for, as I soon after† relinquished the practice of physic, I desisted in a great measure from prosecuting any inquiries connected with medicine.

However, since so much of this subject as is strictly physiological, relating to the natural course of circulating fluids, and more especially so much of the investigation as is conducted by chemical means, is within the range of those pursuits, which are generally interesting to the Royal

* Phil. Trans, for 1811, p 96. † 1800.

Society

Society, I will endeavour to give you as distinct an account as I am able of the progress of my own experiments; requesting that you will in return state, more fully than you have hitherto done, the result of that farther step in the inquiry, which you took at my suggestion; and if it is agreeable to you, we will without delay make a joint communication of our researches to the Society.

Although Dr. Rollo had been assisted in the chemical part of his inquiry by the well known talents of Mr. Cruickshank, it appears, that they "had not been so fortunate as "to obtain a sufficient quantity of serum for chemical experiment*;" and were unable fully to satisfy themselves by the taste, or by other means which they could employ, concerning the existence or nonexistence of sugar in the blood of persons labouring under diabetes; but nevertheless they were persuaded of its presence. Dr. Rollo and Mr. Cruickshank had no opportunity of determining the point directly,

For the purpose of forming some judgment on this question, Mr. Cruickshank made trial of the quantities of oxalic acid, that could be formed from serum or from blood in their natural state, and from the same serum or blood after the addition of a certain proportion of sugar; and from the difference perceptible in these trials, he formed a probable conjecture respecting the presence or absence of sugar in the serum of diabetic persons. but attempted it indirectly.

This method, it is evident, is liable to a twofold objection; first, that an excess of other ingredients beside sugar will cause an increase of the quantity of oxalic acid formed; and secondly, that slight variations in the process for forming oxalic acid will unavoidably occasion differences in the result. Their method objectionable.

The method which I employed appears to me capable of detecting much smaller quantities of such an ingredient; for, though it might not enable us to distinguish exactly the nature of any small quantity that may be discovered, still the mere question of absence or presence admits of determination with great precision. Method employed by the author.

For this purpose I investigated, in the first place, how the albuminous part of healthy serum could be most completely

* Rollo on Diabetes, p. 408.

pletely

pletely coagulated, and by what appearances the presence of sugar that had been added to it would be most easily discerned.

The albumen in serum not completely coagulated by heat,

When heat alone had been employed for the coagulation of serum, to which water had been added, that which exuded from it was still found to contain a portion of albumen dissolved in it; and if this were allowed to remain, any saccharine matter which might be present would be disguised, and could not with certainty be detected.

unless a small portion of dilute acid was previously added.

I found, however, that this residuum of coagulable matter might be altogether prevented by the addition of a small quantity of dilute acid to the serum before coagulation*. To six drams of serum I added half a dram of muriatic acid previously diluted with one dram and a half of water, and immersed the phial containing them in boiling water during four minutes. The coagulation was thus rendered complete. In the course of a few hours a dram or more of water exudes from serum that has been so coagulated. If a drop of this water be evaporated, the salts which it contains are found to crystallize, so that the form of the crystals may be easily distinguished; they are principally common salt.

Salt in the serum.

Effect of the presence of sugar.

If any portion of saccharine matter has been added to the serum previous to coagulation, the crystallization of the salts is impeded, or wholly prevented, according to the quantity of sugar present.

If the quantity added does not exceed two grains and a half to the ounce, the crystallization is not prevented; but even this small quantity is perceptible by a degree of blackness, that appears after evaporation: occasioned, as I suppose, by the action of a small excess of acid on the sugar.

If five grains have been added, the crystallization is very imperfect, and soon disappears in a moist air by deliquescence of the sugar. The blackness is also deeper than in the former case.

* I presumed, that this portion of albumen was retained in solution by the alkali redundant in serum, and added the acid for the purpose of neutralizing it.

By

By addition of ten grains to the ounce, the crystallization of the salts is entirely prevented, and the degree of blackness, and disposition to deliquesce are of course more manifest than with smaller quantities*.

Sugar from diabetic urine had the same effects.

As I was aware, that the sugar obtained from diabetic urine is a different substance from common sugar (approaching more nearly to the sugar of figs), I had the precaution to repeat the same series of experiments upon serum, to which I made corresponding additions of dry sugar, that I had formerly extracted from the urine of a person who voided it in considerable quantity; and I found the effects to be perfectly similar in every respect.

Farther test of sugar.

As a farther test of the absence or presence of sugar, I found it convenient to add a little nitric acid to the salts, that remained after crystallization of the drop. If the serum has been successfully coagulated without any addition of sugar, the addition of nitric acid merely converts the muriatic salts into nitrates, and nitrate of soda is seen to crystallize without foam or blackness. But when sugar has been added, a white foam rises round the margin of the drop; and, if farther heat be applied, it becomes black in proportion to the quantity of sugar present.

Difficulties in experiments.

Such are the appearances, when the proportions have been duly adjusted, and the proper heat for coagulation applied. I must own, however, that I could not always succeed to my satisfaction, at the time when these experiments were conducted; and I am inclined to ascribe occasional failures to having used more muriatic acid than was really necessary, which by excess of heat might redissolve a part of the coagulated albumen, and thence occasion appearances, which, without careful discrimination, might be ascribed to sugar.

After having, by this course of experiment, satisfied myself as to the phenomena exhibited by serum in its natural state, and the effects of any small additions of sugar, I then proceeded to the examination of such specimens of diabetic blood or of serum, as I was able to procure.

* In Plate V, fig. 6, are represented the degrees of blackness of the drop occasioned by adding one, two, three, and four grains of sugar to six drachms of serum.

The

Trial with diabetic blood that had been dried.

The first which I examined was a portion of blood that had been taken from a person, whose urine had been analysed, and found to contain sugar. This blood had been dried, when fresh, by a gentle heat, so as not to coagulate the serum. After being reduced to powder, it was mixed with water, in order that every thing which remained soluble might be extracted. A little muriatic acid was then added, and sufficient heat applied for coagulation of the albumen. The water that separated after coagulation was found to contain the salts of the blood, but no trace whatever of sugar.

No sugar.

2d experiment.

A second specimen of dried blood, that had been ascertained to be diabetic on the same evidence as the preceding, was examined in a similar manner, with the same result, as no appearance of sugar could be discerned.

3d experiment.

In a third instance, I had some serum from the blood of a person, whose urine had been tasted, and found "*very sweet.*" (I had no opportunity of procuring any of this urine for analysis). After a portion of this serum had been coagulated, with the addition of the usual proportion of muriatic acid, there was no appearance whatever of sugar. But when three grains of diabetic sugar had been added to another ounce of the same serum, the presence of this quantity was manifest by the same process.

4th experiment.

I had also a fourth opportunity of examining serum of a person, whose urine contained so much saccharine matter, that an ounce of it yielded, by evaporation, thirty six grains of extract. In this instance I was not so successful in my experiment; for, though I was satisfied that no sugar was present, there certainly was a degree of blackness, which might have been occasioned by about one grain and a half of sugar in the ounce of serum. But this black matter appeared not to be sugar: it was more easily dried than sugar: it was not fusible by heat as sugar is: and its refractive power* was too great for that of sugar.

An equivocal appearance.

I unfortunately had no opportunity of repeating the experiment on a second portion of the same serum, having

* The method by which this was tried has since that time been described in the Phil. Trans. for 1802. See Journal, vol. IV, p. 89.

inconsiderately

inconsiderately employed it for other experiments, and coagulated it at the same time with the former.

5th experiment.

In the next experiment I added half a dram of the urine of the same person to six drams of the serum, and, with a due proportion of diluted muriatic acid, coagulated as before. Although the quantity of extract added did not exceed $\frac{2}{1}\frac{4}{6}$, or two grains and a quarter of extract, the difference was very manifest by the darkness of the colour and the defective crystallization of the salts.

To the remaining quantity of the serum I had added twice the former proportion of the urine, and found that this quantity did not wholly prevent the crystallization of the salts during the evaporation of the drop.

No sugar in serum.

The result of these trials was such, as to satisfy me, that the serum in this instance contained no perceptible quantity of sugar; or, at least, that the water separable from the coagulated serum did not contain one thirtieth part of that proportion, which I had found in the urine of the same person.

The sugar formed in the kidneys,

or conveyed to them directly from the stomach.

In order to account for the presence of sugar in the urine, we must consequently either suppose a power in the kidneys of forming this new product by secretion, which does not seem to accord with the proper office of that organ; or, if we suppose the sugar to be formed in the stomach by a process of imperfect assimilation, we must then admit the existence of some channel of conveyance from the stomach to the bladder, without passing through the general system of blood vessels. That some such channel does exist, Dr. Darwin* endeavoured to ascertain, by giving large doses of nitre, which he could perceive to pass with the urine, but could not detect in its passage through the blood; and he imagined the channel by which it was conveyed to be the absorbent system, upon the supposition, that they might admit of a retrograde motion of their contents.

Dr. Darwin's attempt to prove this by nitre.

The author's by prussiate of potash.

Without adopting the theory of Dr. Darwin, it did appear to me, that the fact deserved to be ascertained by some test more decisive than nitre, and I conceived, that, if prus-

* Account of the retograde motion of the absorbent vessels, by Charles Darwin.

siate

siate of potash could be taken with safety, its presence would be discerned by means of a solution of iron in as small proportion as almost any known chemical test. Upon trial of this salt, I found, that a solution of it might be taken without the least inconvenience, and that in less than one hour and a half the urine became perceptibly impregnated, and continued so to the fifth or sixth hour, although the quantity taken had not amounted to more than three grains of the salt.

Experiment.

After a few previous trials of the period, when the principal impregnation of the urine might be expected, and when the presence of the prussiate (if it existed in the blood) might with most reason be presumed to occur, a healthy person, about thirty-four years of age, was induced to take a dose corresponding to three grains and a half of the dry salt, and to repeat it every hour to the third time. The urine, being examined every half hour, was found in two hours to be tinged, and to afford a deep blue at the end of four hours. Blood was then taken from the arm, and the coagulum, after it had formed, was allowed to contract, so that the serum might be fully separated. The presence of the prussiate was then endeavoured to be discovered by means of a solution of iron, but without effect: and as I thought, that the redundant alkali (which had been ascertained to prevail in this serum) might tend to prevent the appearance of the precipitate, I added a small quantity of dilute acid; but still I could not discern, that any degree of blueness was occasioned by it.

The experiment repeated.

This experiment, having been repeated a second time with the same result, seemed to me nearly conclusive with respect to the existence of some passage, by which substances certainly known to be in the stomach may find their way to the bladder without being mixed with the general mass of circulating fluids.

No prussiate found in the saliva,

Being desirous of ascertaining, whether the prussiate could be discovered in any other secretions, I have repeatedly examined my saliva, at times when the urine has manifested a very strong blue, by adding solution of iron, but I could at no time perceive the saliva to be tinged.

or aqueous

I have also, during a severe cold, accompanied with profuse

fuse running of water from the nose, made a similar examination of this discharge, but have not been able to perceive any trace of the prussic acid.

Discharge from the nose.

It was nearly in this state, that I left the inquiry at the period I have mentioned; and I do not remember to have made any other experiments, when I requested your assistance in making trial of the serum, that is secreted in consequence of the application of a blister. Your report upon the result of your experiments, in addition to those which I have above related, nearly satisfied me as to the existence of some unknown channel of conveyance, by which substances may reach the bladder.

Trial of the serum from a blister.

With respect to Dr. Darwin's conception of a retrograde action of the absorbents, it is so strongly opposed by the known structure of that system of vessels, that I believe few persons will admit it to be in any degree probable.

Retrograde action of the absorbents improbable.

Since we have become acquainted with the surprising chemical effects of the lowest states of electricity, I have been inclined to hope, that we might from this source derive some explanation of such phenomena. But though I have referred* secretion in general to the agency of the electric power with which the nerves appeared to be indued, and am thereby reconciled to the secretion of acid urine from blood that is known to be alkaline, which before that time seemed highly paradoxical; and although the transfer of the prussiate of potash, of sugar, or of other substances, may equally be effected by the same power as acting cause; still the channel through which they are conveyed remains to be discovered by direct experiment.

Secretion generally effected by electricity.

The channel still to be discovered.

I have, indeed, conjectured, that, by examining the blood in the abdominal vessels, or contents of the lacteals, it might be possible to detect them *in transitu*: but I have not been inclined to make such experiments on living animals, as would perhaps throw light upon the subject.

I remain, dear sir, with great regard,

Yours very truly,

January 1, 1811. W. H. WOLLASTON.

* Philosophical Magazine for June 1809.

Y.

V.

Dr. MARCET's *Reply to Dr.* WOLLASTON, *on the same Subject.*

Russell Square, January 8, 1811.

MY DEAR SIR,

The hypothesis of the presence of sugar in diabetic blood specious.

I Am much gratified to find, that you have at last been induced to communicate to the Royal Society your curious inquiry respecting the state of the blood in diabetes. I was anxious, that the specious hypothesis of the presence of sugar in diabetic blood, which had been sanctioned by the authority of Dr. Rollo and Mr. Cruickshank, and which I had myself urged in support of their theory, fourteen years ago, in an inaugural publication, should no longer obtain an undue weight among physiological inquirers.

Attempt to ascertain whether prussiate of potash, taken internally, be present in any other secretion beside urine.

With regard to the experiments which I tried at your request some years ago, with a view to ascertain whether prussiate of potash taken into the stomach, and found to exist in the urine, could also be detected in other secretions, I find, on referring to my memorandums, the following particulars, which I shall transcribe verbatim.

" August 19, 1807. Having heard from Dr. Wollaston, that prussiate of potash could be taken into the stomach with perfect safety, and that its presence could afterward be discovered in the urine, but not in the serum; and being invited by him to follow up this inquiry, with a view to connect it with the theory of diabetes, I tried the following experiments.

Exp. 1. Prussiate of potash may be taken in considerable doses.

Exp. 1. " After having satisfied myself, by trials made by some medical gentlemen upon themselves, that considerable doses of prussiate of potash might be taken without the least inconvenience, I gave to a young woman, labouring under diabetes mellitus, five grains of prussiate of potash dissolved in water, and this was repeated every hour, till she had

had taken thirteen or fourteen such doses. After the fifth dose, her urine, by the addition of a drop or two of a solution of sulphate of iron, turned blue instantly. At this period of the experiment, a blister was applied to her stomach, and after a few hours, while still taking the prussiate of potash, and while the urine strongly indicated its presence, the blister was cut, and the serum collected. This serous fluid being, in the same manner as the urine, subjected to the action of a solution of sulphate of iron, did not suffer any change of colour in the least indicative of the presence of prussic acid. Yet the urine still remained capable of imparting a blue colour to solution of iron, fifteen hours after taking the last dose of the prussiate of potash.

None in the serum from a blister.

Exp. 2. "The same person being soon afterward put upon a course of ferruginous medicines, and having taken considerable quantities of sulphate of iron, an idea naturally occurred to me, that the phenomenon might perhaps be reversed; but upon adding prussiate of potash to the urine, no vestige of iron could be discovered, and the same attempt was repeated several times with the same negative result.

Exp. 2. Sulphate of iron taken.

No iron in the urine.

Exp. 3. "Dec. 2, 1807. The fluid obtained by means of a blister (as in Experiment 1.) being not immediately derived from the circulation, since it may be considered as the product of a secretion, I was desirous of repeating Dr. Wollaston's experiment on the serum itself, under circumstances of impregnation similar to those, in which the serum of the blister was examined.

Exp. 3.

"For this purpose, a young woman, after taking, in divided doses, about a dram of prussiate of potash in the course of twelve hours, lost some blood by cupping, an operation which had been ordered for a local complaint under which she laboured. The serum having been allowed to separate, and a little nitric acid having been added to it, not the least vestige of prussic acid appeared on applying the test of sulphate of iron, although the urine, made during the six hours which preceded and followed the cupping, was strongly impregnated with that acid, and struck a vivid blue upon adding the smallest quantity of iron."

Prussiate of potash detected in the urine, but not in the serum of the blood.

I have only to observe, in addition to these particulars, that the susceptibility, by which prussiate of potash is transmitted

Prussiate of potash not conveyed to

the bladder in all persons with the same facility.

mitted to the bladder, seems to vary in different individuals; for in five trials, made at Guy's Hospital in Nov. 1805, I failed of discovering any vestige of this salt in the urine of persons, who had taken it in quantities sufficient to produce its appearance in others. Three of these individuals, I should observe, were at the time under mercurial treatment; and an idea occurred to me, that, mercury having a great affinity for prussic acid, the presence of this metal in the system might prevent the effect in question. But, as in the two other failures no mercury was present, I cannot lay any stress upon this conjecture. It may be proper to mention, that, in the frequent trials which I have made with the prussiate of potash, no symptom or inconvenience whatever has ever occurred, which could be ascribed to this salt.

No inconvenience from taking it.

I remain ever,
My dear sir, with great esteem,
Yours sincerely,
ALEX. MARCET.

The channel of conveyance may be the arteries and lymphatics, to the exclusion of the veins.

P. S. While revising the proof of this sheet, it has been observed to me by some friends, and in particular by Dr. Henry of Manchester, and Dr. R. Pearson of London, that, in order to show distinctly that certain substances find their way to the bladder, without passing through the general circulation, it would be necessary to examine the arterial, as well as the venous blood, since it is not impossible, that the whole of the sugar in diabetes, or the prussiate of potash in the experiments above related, may be conveyed to the urinary organs by the arteries, without entering the venous system. According to this hypothesis, it may be conceived, that the same substances, when conveyed by the arteries to distant parts of the body, may return by the absorbent system, and might in this case be discovered in the thoracic duct. This view of the subject may deserve farther investigation; and I hope, that this curious question will soon be decided by appropriate experiments.

VI.

VI.

On the Algorithm of Imaginary Quantities. In a Letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

As neither of the papers which you did me the honour to insert in your Journal, "On the Defective Algorithm of Imaginary Quantities*," has been noticed by any of your mathematical correspondents, I now propose to fulfil my promise, viz. "That if no answer appeared within three months, I would then, through the medium of your Journal, give my own explanation of the difficulty." I cannot, however, but regret, that, amongst the many able advocates for the introduction of these quantities into mathematical investigations, no one of them should have stepped forward to explain an anomaly attending such introduction, which seems to militate so much against the boasted certainty of results, deduced from mathematical investigations.

Defective algorithm of imaginary quantities.

The defect pointed out in my former letters is, I believe, a new case, which has not been noticed by any of the writers on the subject; and, unless it can be satisfactorily answered, it is impossible to deny, that uncertainty and doubt must necessarily attend all results obtained through the medium of imaginary quantities. I am sorry, through the backwardness of your other correspondents, it has fallen to me to enter upon this subject; I will, however, do my best to explain the anomaly in question; at the same time I cannot but again repeat my regret, that it has not been taken up by some one more competent to the task.

Results from imaginary quantities uncertain.

First then, let us consider the origin of these imaginary expressions, which may be traced to the very first principles of algebra. We are taught in multiplication, that $(+a)^2$, and $(-a)^2$, are both to be represented by a^2; and consequently, when the converse of this operation arises, or

Origin of imaginary expressions.

* Vol. XXIX, p. 254, and XXX, p. 209.

when we are required to extract the square root of a^2, it is either $+a$, or $-a$; but if it be required to extract the square root of $-a^2$, we must necessarily be stopped in our progress, for there being no previous convention, that $-a^2$ shall represent the square of any quantity; it follows, that, when such a case arises, we are not able to assign to it any particular root, and we are thus presented with the first and the most simple form of imaginary quantities. These expressions, however, though they have no definite value, yet, considered as mathematical symbols, ought to be subject to certain rules, as well as other symbols, which are the representatives of real quantities; the rules, however, which are laid down for operating in the latter case, frequently require certain modifications before they can be applied to the former, and most of the difficulties, which have occurred with regard to the algorithm of imaginaries, have arisen in making rules general, which were first intended to answer only in particular cases.

Difficulties from applying generally what was intended for particular cases.

The ambiguity does not exist in certain cases.

We have seen above, that $\sqrt{a^2} = \pm a$; that is to say, it has an ambiguous root, which may be taken either at $+a$, or $-a$; but this ambiguity has not place, if we know how the quantity a^2 was generated, and have occasion afterwards to retrace the steps of our operation: we cannot, for instance, say, that $\sqrt{-a \times -a} = \sqrt{a^2} = \pm a$; or that $\sqrt{+a \times +a} = \sqrt{a^2} = \pm a$ for the square root of a in both these cases is determined; that is, when considered with regard to its generation, it has but one root; whereas its origin not been known, we must have prefixed the ambiguous sign to the root a, and for this obvious reason, that we know not, when a^2 is unconditionally assumed, whether it be the representative of $(+a)^2$, or of $(-a)^2$; these being both expressed by the same symbol a^2. In fact, there is no ambiguity in the extraction of the roots of quantities, except in those cases in which we are unacquainted with their generation; and in these it must necessarily arise, because we have agreed to represent the powers of different quantities by the same symbol.

Caused by expressing different quantities by one symbol.

This illus

There are, for instance, three different quantities, which, being

being cubed, give unity for the result; we say, therefore, that 1 has three roots; and, if we are simply requested to extract the cube root of 1, we may make it either 1 or $-\frac{1}{2}+\frac{1}{2}\sqrt{-3}$, or $-\frac{1}{2}-\frac{1}{2}\sqrt{-3}$; for the cube of each of these quantities is represented by 1. But if we are asked, what is the cube root of $(-\frac{1}{2}+\frac{1}{2}\sqrt{-3})^3$, we must not say, that $(-\frac{1}{2}+\frac{1}{2}\sqrt{-3})^3=1$, and therefore its root is also equal to 1; for, as in this case we know, that 1 is the representative of the cube of a particular quantity, its cube root must necessarily be that quantity, and no other. trated by an example.

Hence it appears, that there is no ambiguity in extracting the roots of quantities, which are known to be generated from the constant multiplication of a given quantity by itself, whether this quantity be real or imaginary. But if, by the multiplication of two unequal factors, we arrive at any result, and have afterwards to extract the root of that quantity, there is then nothing in the nature of the case to limit the root. If, for example, we find, that the product $(-\frac{1}{2}+\frac{1}{2}\sqrt{-3})\times(-\frac{1}{2}-\frac{1}{2}\sqrt{-3})=1$; and we have afterwards to take the cube root of this product; there is nothing to indicate, that we ought to take one root in preference to another: whence it follows, that a quantity generated from the product of unequal factors has all the generality of a quantity unconditionally assumed; whereas that which is generated from the product of equal factors has not that generality, as it admits only of one root; while the other quantity, made up of unequal factors, has as many roots as there are units in the index of the power, the root of which is to be extracted.

No ambiguity when we know how the quantity was generated, unless it was the result of unequal factors.

Let us now see how far what has been said above will serve to explain the difficulty stated in my former letters, or rather in my last letter, to which I intend more particularly to direct the following remarks:

Application to the point in question.

The quantity which I proposed to square was the following,

$$\sqrt[3]{-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}}+\sqrt[3]{-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}}.$$

Now here the quantities under the cubic radicals are the two imaginary roots of the equation $x^3=1$; which, for the

 sake

Answer to the question on imaginary quantities.

the reason of this restriction is obvious; because though $a^2 = b$; and consequently $a^3 = ab$, which are both represented by unity, or 1, yet the cube root of the former is a, and of the latter 1: as is evident from the preceding part of this paper, the first (1,) being produced from the cubing of a certain quantity a, and therefore having its cube root $= a$; and the other, being the product of two unequal factors, possesses all the generality of 1 unconditionally assumed.

Hence it appears, that there may be quantities equal to each other, which, when placed under the same radicals, lose their equality: the one of them being restricted to give a particular root, and the other a different root; and therefore, in such cases, although the quantities and the radicals are precisely the same, yet all equality between the two ceases, and they must be considered as totally dissimilar quantities.

This is exactly the case in the examples proposed, as may be made evident as follows:

We have seen, that $a^2 = b$, and $b^2 = a$; and if, in consequence of this equality, we make our first result, obtained from squaring, viz.

$$\sqrt[3]{a^2} + 2 + \sqrt[3]{b^2} =$$
$$\sqrt[3]{b} + 2 + \sqrt[3]{a}$$

as we did in the numerical example, we should find, that in continuing the operation, we should not arrive at the same result as in the former case; that is, by multiplying again, in order to cube the expression, we should have

$$\begin{array}{l}
\sqrt[3]{b} + 2 + \sqrt[3]{a} \\
\sqrt[3]{a} + \sqrt[3]{b} \\
\hline
\sqrt[3]{ab} + 2\sqrt[3]{a} + \sqrt[3]{a^2} \\
\qquad + \sqrt[3]{b^2} + 2\sqrt[3]{b} + \sqrt[3]{ab} \\
\hline
\end{array}$$

$$1 + 3\sqrt[3]{a} + 3\sqrt[3]{b} + 1 =$$
$$3(\sqrt[3]{a} + \sqrt[3]{b}) + 2$$

which is different from the former result.

The

The difficulty therefore stated in both my former letters is thus explained, namely, that we have considered quantities as equal, because they are represented by the same symbols; whereas, in consequence of thus entering under radicals, and being restricted with regard to their roots, all equality between them ceases.

With regard to the proper criterion, by which such anomalies may be guarded against in other cases, there appears to be none more general than the following, viz.—that, in the involution of quantities under radicals, the operation should not be worked out at length, but indicated by the sign of the particular power, to which it is to be raised. Method of guarding against such anomalies.

I have thus endeavoured to fulfil the promise which I made in my former letter; but how far what I have said may be considered as satisfactory must be left to others to decide. I shall only observe, that if any of your correspondents should perceive any defect in the reasoning employed in the preceding pages, I shall be extremely happy to see it corrected in a subsequent number of this Journal; if not, I may probably at some future time trouble you with a few other remarks on this subject,

Yours,

MATHEMATICUS.

VII.

Description of a Compensation Pendulum for a Clock: by Mr. Adam Reid, *of Green's End, Woolwich**.

SIR,

YOU will have the goodness to lay before the Society of Arts &c. a half-second compensating pendulum of my invention; which is so simple in its construction, that it will be fully understood by viewing either the pendulum, or the drawing Half-second compensation pendulum.

* Trans. of the Soc of Arts, vol. XXVIII, p. 230. Fifteen guineas were voted to Mr. Reid for this invention.

drawing which accompanies it. I believe it to be new, and wishing it to be useful to the world, I have presumed to send it to the Society. I am, sir,

Your humble servant,

ADAM REID.

Reference to the Drawing of Mr. Reid's Compensation Pendulum, Pl. VI.

The pendulum described.

Fig. 1 and 2, Pl. VI, A B represent the steel rod extending through the whole. C the bob, supported upon the compensating cylinder of zinc D, which surrounds the rod A B, and rests upon the nut E of a screw tapped upon the end of the steel rod, to bring it to exact time; as this expands downwards by heat, the zinc expands upwards the same quantity; so that the bob always remains at the same distance from the point of suspension. Fig. 2, is a section to explain more clearly the thickness of the zinc tube D, and the form of the steel rod at *a*, where it passes through the bob, which is of the shape shown at L, that the rod or the bob may not turn round, when the nut E is turned to adjust it to time.

The following Method is to be employed to make the Compensating Pendulum of Steel and Zinc.

Method of making it.

Procure a rod of forged blister-steel 52·7 inches long, ·27 diameter, heat it to a white heat, which will open the pores of the steel, and give it the smallest expansive power, that steel of this texture is possessed of. When cold straighten it with a mallet of wood on a wooden block, that no part may be condensed partially; which would be the case, if a hammer and anvil were used. Then cast a solid rod of zinc 12·5 inches long, ·68 diameter, with the lowest heat, that will fuse it; and pour it into a metal mould. This will give it the greatest density, consequently the greatest expansion, that zinc is possessed of. Then bore a hole through the centre of it longitudinally, that it may move freely on the steel rod, which has a nut and screw at the bottom end to regulate the clock to time; the bob, as shown in the engraving, rests on the upper end of the cylinder of zinc, and will continue in the same place, whatever expansion or contraction takes place, if the adjustment be correct.

If

If platina was used instead of steel, and steel instead of zinc, a pendulum might be made equally good, and more compact; but not at so small an expense. Platina & steel still better.

The above dimensions are to be understood in the finished state of their diameters and lengths, proper for a second pendulum. The dimensions above for a second pendulum.

I have constructed a pendulum on this principle, which has been in use some months, and I have the satisfaction to find it has answered my expectations; the temperature of the room was from 58 to 34 degrees, and no variation of the clock, when compared with my other clock, which, from many years trial, I know to be a good one, in a room where the thermometer does not vary more than four degrees. Answered on trial.

The difficulty, or rather impossibility of making a good pendulum, where a compound metal as brass is employed, arises from the circumstance, that neither brass nor any compound metal can be made uniform, not even for one foot in length; and then if drawn into wire the parts acquire a longitudinal grain, which adds to the variation of the expansive powers. To avoid this, zinc has been substituted, with no more certainty of success; for if a pendulum is made in the summer, and the steel pins fill the holes well, and it is exposed to a severe frost, and put to the clock for only a few years, the invisible fissures will become visible in the performance of the best clock, and a visible separation render the pendulum useless; as I have witnessed. Inconvenience of a compound metal, and of zinc.

I am willing to furnish the Society with any farther information in my power upon the subject.

Woolwich, April 27, 1809. ADAM REID.

VIII.

Method of ascertaining the Hour in the Night, by an Apparatus connected with a common Watch: by Mr. G. SPARK, *of Elgin, Murrayshire, Scotland**.

SIR,

BY Mr. John Newton, watchmaker, I have forwarded an invention for knowing the hour in the dark, by feeling. Invention for knowing the

* Trans. of the Soc. of Arts, vol. XXVIII, p. 234. The silver medal was voted to Mr. Spark for this invention.

I think

hour in the dark.

I think it preferable to a repeater, on account of its simplicity and cheapness. It is not liable to be out of order, and it does not require the exertion necessary for pushing the pendant of a repeater, or disturb any person near it.

For these reasons I diffidently wish to have the honour of laying this invention, which I call a Noctuary, before the Society; and to be favoured with their decision on its merits.

I am, sir,

Your respectful and obedient servant,

Elgin, March 7, 1810. GEORGE SPARK.

Reference to the Engraving of Mr. Spark's Noctuary, Pl. VI, Fig. 3, *and* 4.

The apparatus described.

A A, fig. 3, is a mahogany board, upon which two others, B and C, are fixed, so as to form a groove beneath and underneath them, in which the index D, shown separately at fig. 4, descends.—On the opposite side of the board a flap or door of mahogany is fixed by two hinges, *a a*, and a clasp; between this and the board A A a cavity is formed to contain the watch, as shown by the dotted circle X; the dial appearing through a circle in the door, a hole is made through the board A, opposite the fusee square, to receive a key, upon which the small pulley E is fixed, and from which the index D is suspended by a fine thread in the groove above-mentioned. It is plain, that, as the fusee-square and pulley E revolve, the index descends, and points out the hour by coming opposite the several marks, commencing at nine and ending at seven, which are fixed upon the board B; the marks, from twelve to seven, are made by pins projecting from the surface, so as to be readily distinguished by the finger; the index, represented in perspective at L, fig. 4, is made very light, that it may not influence the motion of the watch. The watch must be wound up before it is placed in the frame, and the thread wound up on the pulley E, so much as to suspend the index nearly the height of the hour when it is set; the key is then pushed on the fusee-square, and if the index does not point exactly at the right hour, the scale B can be slided up or down to adjust it; the screws *d d*, which hold it, being fitted in grooves for this purpose; after this setting, the index will point out

any

any succeeding hour, descending a division at each. F is the ring by which the instrument is suspended; and G is a hole, in which the key on the pulley E is placed, when the watch is removed, and the instrument out of use.

IX.

On the Management of the Onion. By THOMAS ANDREW KNIGHT, *Esq. F. R. S., &c.**

THE first object of the Horticultural Society being to point out improvements in the culture of those plants, which are extensively useful to the public, I send a few remarks on the management of one of these, the onion; which both constitutes one of the humble luxuries of the poor, and finds its way in various forms to the tables of the affluent and luxurious. Culture of useful plants a prime object.

Every bulbous rooted plant, and indeed every plant which produces leaves, and lives longer than one year, generates, in one season, the sap, or vegetable blood, which composes the leaves and roots of the succeeding spring; and when the sap has accumulated during one or more seasons, it is ultimately expended in the production of blossoms and seeds. This reserved sap is deposited in, and composes in a great measure, the bulb; and moreover the quantity accumulated, as well as the period required for its accumulation, varies greatly in the same species of plant, under more or less favourable circumstances. Thus the onion in the south of Europe acquires a much larger size during the long and warm summers of Spain and Portugal, in a single season, than in the colder climate of England; but under the following mode of culture, which I have long practised, two summers in England produce nearly the effect of one in Spain or Portugal, and the onion assumes nearly the form and size of those thence imported. Growth of bulbs. The onion.

* Trans. of the Horticultural Society, vol. I, p. 157.

Seeds

method of obtaining onions equal to the Spanish,

Seeds of the Spanish or Portugal onion are sown at the usual period in the spring, very thickly, and in poor soil; generally under the shade of a fruit tree: and in such situations the bulbs in the autumn are rarely found much to exceed the size of a large pea. These are then taken from the ground, and preserved till the succeeding spring, when they are planted at equal distances from each other, and they afford plants, which differ from those raised immediately from seed only in possessing much greater strength and vigour, owing to the quantity of previously generated sap being much greater in the bulb than in the seed. The bulbs, thus raised, often exceed considerably five inches in diameter, and being more mature, they are with more certainty preserved, in a state of perfect soundness, through the winter, than those raised from seed in a single season, The same effects are, in some measure, produced by sowing the seeds in August, as is often done; but the crops often perish during the winter, and the ground becomes compressed and soddened (to use an antiquated term) by the winter rains; and I have in consequence always found, that any given weight of this plant may be obtained, with less expense to the grower, by the mode of culture I recommend, than by any other which I have seen practised.

and excellent for keeping.

X.

Hints relative to the Culture of the Early Purple Brocoli, as practised in the Garden of Daniel Beale, Esq. at Edmonton. By Mr. JOHN MAHER, F. H. S*.

Brocoli much improved.

FEW vegetables have been more improved of late years than brocoli, so that it now almost equals in flavour and magnitude the delicate cauliflower, over which it has the decided advantage of being more hardy, and may, by a little management, be procured through the whole winter.

Whiteness a perfection in plants of this family.

Several varieties, differing in colour from white to deep purple, are sold by our nurserymen; and as all plants of this natural family become less alkalescent and more pala-

* Trans. of the Horticultural Society, Vol. I, p. 116.

table

table in proportion as they approach to a pale or white colour, such varieties will undoubtedly be preferable to purple ones, if they turn out equally hardy: nor are we to despair of raising them, by patience and perseverance in selecting the largest and whitest specimens of the common brocoli for seed.

Difficulties in the pursuit.

All attempts of this kind however demand both a long time, and no trifling expense; nor can they be easily prosecuted, except in the insulated grounds of those gentlemen, whose liberality, like that of my master, rivals their extensive possessions: for, out of a great number of plants set apart for seed, perhaps not even one may answer our wishes; and if a* brisk gale of wind, or wandering bees, bring the pollen of any other variety to their flowers, the progeny, in ninety-nine instances out of a hundred, will be deteriorated instead of improved, and in no case prove the identical variety sown.

Excellent brocoli of a large size.

The brocoli, of which I am now emboldened to offer some account to the Horticultural Society, is reported to have been introduced from the Cape of Good Hope, by the Hon. Marmaduke Dawnay, and first cultivated in Surry, where it is called the early Cape brocoli. Packets of seed, first sent here from Italy, which appear to me to have produced the same variety, have also been sold for two seasons by Mr. Grange, fruiterer, in Covent Garden and Piccadilly: it may therefore easily be obtained, and our principal care now must be to preserve it in its present magnitude and excellence.

Time of sowing the seeds.

My method of treating it is as follows. Three crops are sown annually: the first between the 12th and 18th of April: a second between the 18th and 24th of May: the third between the 19th and 25th of August: these successive crops supply the family from September till the end of May.

* The result of an action for damages brought in Westminster Hall more than a century ago, against an innocent but unfortunate gardener, for selling cauliflower seeds, which only produced long-leaved cabbages, has been stamped with immortality by the pen of Linneus, in his celebrated treatise on the sexes of plants, the Sponsalia Plantarum, and confirms this remark of the author's very forcibly. Seer.

The

Sowing, and planting out.

The seeds are scattered exceedingly thin, in a border of very rich light earth. Not a weed is suffered to appear; and when the young plants have from 8 to 10 leaves, which is in about a month, they are finally planted out at the distance of two feet every way, in a piece of sandy loam, which has been well prepared for the purpose by digging, and enriching it with a large portion of very rotten dung, frequently turned over to pick out every sort of grub, or insect deposited in it. The ground is kept constantly clean by hoeing, whenever a seed leaf of any weed springs up; and the loose surface is drawn together into a heap, round the stem of each plant.

Second crop for winter use.

The second crop is treated exactly as the first; but the weaker plants left in the seed bed are permitted to remain 8 or 10 days longer, to gain more strength. They are then transplanted into pots of the size called sixteens, filled with very rich compost, placing them close to each other in the shade, and duly watering the plants, till they begin to grow freely. After this, the pots are plunged in the open ground at two feet distance from each other every way, and about 3 inches under the general level, leaving a hollow or basin round each plant, to retain any water given to them when necessary. By the time the pots are filled with roots, and the autumnal rains render watering unnecessary, the basins are filled up by drawing the earth round each plant, at the same time pressing it firmly down, to prevent the wind from shaking them. A few of these plants in pots sometimes show flowers too soon, and to guard them from early frost, a leaf or two is broken down over them. On the approach of settled frost in December and January, all the pots are taken up and removed to a frame, pit, or shed, where they can be sheltered from the extreme severity of the winter, but have air when it is milder, and by this method a supply is preserved for the table in the hardest winters. To make brocoli succeed in pots, I find by experience, that it should be potted immediately from the seed bed. If it is transplanted oftener, the head or flower is both less in size, and runs much sooner after it forms. For the same reason, I never prick out or transplant the general crops; and as the temperature of our climate

Transplanting injurious.

climate does not suffer vegetation to go on briskly from October to March, by following this method, the heads of flower will remain a long time in a state of rest after they are formed without bursting, and heads from 6 to 7 inches diameter are the ordinary produce of our plants.

Third crop. The seeds of the third crop are sown in a frame, or under hand glasses; and about the third week in October the plants become strong enough to remove, as in the two former crops. **Seed plants.** From this sowing, the best plants are selected for seed, and placed 3 or 4 under a hand glass according to its size; 3 however are sufficient, for they should not afterward be disturbed. They are gently watered and covered, till they have made fresh roots; after which air is plentifully admitted, treating them through the winter exactly like cauliflower plants. From the hints already given, it may be deduced, that these seedling plants should not only be placed in a part of the garden remote from every other variety of the cabbage tribe, but that no plant whatever of any variety, except it is wanted for seed, should be suffered accidentally to show a flower in the garden; and this business in the months of May and June, when two or three hot days often produce the effects of apparent enchantment, by suddenly bringing radishes, turnips, boorcole, cabbages, sea kale, and cauliflowers into bloom, requires very strict attention on the part of the gardener. **General caution respecting seedling plants.**

XI.

On some Exotics, which endure the open Air in Devonshire. In a Letter to the Right Hon. Sir JOSEPH BANKS, *Bart. K. B. &c. By* A. HAWKINS, *Esq**.

SIR,

THOUGH I have no knowledge of the Horticultural Society, but through the medium of extracts in the last Monthly Review, (which informed me of its existence), yet, struck with your "Hints respecting the proper Mode of inuring tender Plants to our Climate," and residing in the very warmest part of England (the South Hams of Devonshire, of which I am a native), within view of an inlet of the sea, **Exotics growing in the open air in Devonshire.**

* Trans. of the Hort. Soc. vol. I, p. 175.

sea, I am led to state to you some facts, that perhaps may not be wholly unworthy of notice.

Camellia japonica.

In October, 1795, a camellia japonica was planted here among other shrubs in the open ground; it has stood every winter since, without the smallest shelter, thrives well, and has never had a branch or leaf injured by the weather; it is now about four feet high, the size of a gooseberry bush, but has not flowered.

Fuchsia coccinea.

Two plants of the fuchsia coccinea were planted about four years ago under a brick wall facing the south. At first the branches suffered by the frost, but they put forth new shoots in the spring, with much strength, and have flowered well every summer. During the last two years I was absent, but I understand, that only the extremities of the branches were injured, and they have always flowered in great perfection.

Solanum pseudocapsicum.

Some plants of solanum pseudocapsicum, or amomum Plinii, are also under a brick wall, (but not nailed against it) which have stood many years, and only a small part of the very extremities of their branches has been injured by frost.

Myrtles.

Myrtles of every kind (even the doubled blossomed and orange) do exceedingly well in the open ground, though the silver, from the richness of the soil, soon becomes plain*.

Buddlea globosa.

The buddlea globosa likewise stands the climate; and some of the plants are ten feet high, spread wide, and make a handsome appearance. One of them is placed in a situation open to the north-east winds, where the sun cannot shine during the short days, yet it has stood there since 1794, and never had more than the extremities of the branches hurt.

American aloe.

About two miles from my house is the small seaport town of Salcombe, just between those two well known points, the Prawl and Bolt-head, the latter of which is in the parish whence this letter is written, a place that the sea washes on three sides. Perhaps of all spots in the British isles, Sal-

* I have seen myrtles, as far up the channel as Weymouth, both broad and narrow leaved, at least twelve or fifteen feet high, trained against walls in the open air, as jessamine commonly is. C.

combe

combe is the very first for climate and shelter. The celebrated Doctor Huxam used to call it the Montpellier of England. In 1774, a large American aloe, only twenty-eight years old, and which had always stood in the open ground, without covering, flowered there; it grew to the height of twenty-eight feet, the leaves were six inches thick, and nine feet in length, and the flowers, on forty-two branches, innumerable.

Several plants of the verbena triphylla are growing at Salcombe in the open ground, and are now six feet high. I have not tried any of them myself; but as I expect to be more at home in future, than for some years past, I shall not fail to add this plant to those tender shrubs already growing around me. Three leaved vervain.

Oranges and lemons, trained as peach trees against walls, and sheltered only with mats of straw during the winter, have been seen in a few gardens of the south of Devonshire for these hundred years. The fruit is as large and fine as any from Portugal; some lemons from a garden near this place were, about thirty-five or forty years ago, presented to the King by the late Earl Poulett, from his sister Lady Bridget Bastard, of Gerston; and there are trees still in the neighbourhood, the planting of which, I believe, is beyond memory. The late Mr. Pollexfen Bastard (uncle of the M. P. for Devon,) who had the greatest number of oranges and lemons of any one in this country, remarked above thirty years since (what tends to confirm your experiments), that he found trees raised from seed, and inoculated in his own garden, bore the cold better than oranges and lemons imported. Oranges and lemons. Trees from seed bear the cold best.

I have the honour to be,

Sir, your very obedient Servant,

A. HAWKINS.

Alston, near Kingsbridge, Devon,
December, 11, 1809.

XII.

On a new variety of Pear. By THOMAS ANDREW KNIGHT, *Esq, F. R. S. &c**.

Remarks on the ripening of the pear.

HAD the pear been recently introduced into England from a climate similar to that of the south of France, in which it had been found to ripen in the months of August and September, and to become fit for the dessert in the four succeeding months, it might have been inferred, with little apparent danger of errour, that the same fruit would ripen here in October, and be fit for our tables during winter; provided its blossoms proved sufficiently hardy to set in our climate. But had many varieties of this fruit been proved by subsequent experience to be capable of acquiring maturity before the conclusion of our summer, and in the early part of the autumn, without the aid of a wall, scarcely any doubts could have been entertained of the facility of obtaining numerous varieties, which would ripen well on standard trees to supply our tables during winter; for it would be very extraordinary, if the whole of our summer, and of our long, and generally warm autumn, would not effect that, which a part of our summer alone had been proved to be capable of effecting; nevertheless, though varieties of the pear abound, which bear and ripen well in the early part of the autumn, we possess scarcely any good winter pears, which do not require an east or west wall, in the warmer parts of England, and a south wall in the colder parts. This can arise only from the want of varieties; and I venture most confidently to predict, that (if proper experiments be made to form such varieties) winter pears, of equal merits with those which now grow on our best walls, will be obtained in the utmost abundance from standard trees; and that such pears may be sold with sufficient profit to the grower, on as low terms as apples are now sold, during winter: for I have had several opportunities of observing, that the fruit of seedling pear trees generally bears a considerable resemblance to that of their parent trees; and the experiments I have made on other species of

We have no good winter pears that are standards.

But such might be obtained, and the pears sold as cheap as apples,

* Trans. of the Horticultural Society, vol. I, p. 178.

of fruits induce me to believe, that a good copy of almost any varieties may be obtained; and as I have more than once succeeded in combining the hardiness and vigour of the yellow Siberian crab, with the richness of the golden pippin, I do not doubt of the practicability of combining the hardiness of the swan's egg pear with all the valuable qualities of the colmar, or bezi de Chaumontel, and I consider the climate of England as peculiarly well calculated for the necessary experiments*.

Cheap fruits important in a national view.

I am disposed to annex some degree of importance to the production of abundant crops of fruit to supply our markets, at a moderate price, during the winter and spring; for it has been often observed, that great manufacturing towns have been generally more healthy in seasons, when fruits have abounded, than in others; and the same palate, which is accustomed to, and pleased with sweet fruits, is rarely found to be pleased with spirits, or strong fermented liquors: therefore, as feeble causes, which are constantly operating, ultimately produce very extensive effects on the habits of mankind, I am inclined to hope, and to believe, that markets abundantly supplied at all seasons with fruits would have a tendency to operate favourably, both on the physical and moral health of our people.

Attempts to effect this.

Under these considerations, I have amused myself with attempts to form new varieties of winter pears; and though my experiments are yet in their infancy, and I have seen the result of one only, and that under very unfavourable circumstances, I am induced to state the progress, that I have made, to the Horticultural Society, in the hope that others will join me in the same pursuit.

Experiment.

In the spring of the year 1797, I extracted the stamina from the blossoms of a young and vigorous tree of the autumn bergamot pear, which grew in a very rich soil; and I introduced, at the proper subsequent period, the pollen of the St. Germain pear, and from this experiment I obtained several fruits, with ripe seeds: I, however, succeeded in raising only two plants. One of these was feeble and dwarfish in its growth, as well as wild and thorny in its appearance, and I did not think it worth preserving. The other

* See Hort. Trans., vol. I, p. 30: or Journ. vol. XVIII, p. 189.

 presented

presented a much more favourable character; and I fancied that I could discover in it some traces of the features of its male parent. This plant afforded blossoms in the spring of 1808, but I had very unfortunately removed it from the seed bed, when it was fourteen feet high, in the preceding winter, and as it had never been previously transplanted it had retained but very few roots. Two of the blossoms, nevertheless, afforded fruit; which began to grow with rapidity as soon as the tree had emitted new roots, but this was not till late in the summer; and on the 8th of October the fruit was blown from the tree by a violent storm. The two pears were then very nearly of the same weight and size, each being somewhat more than eight inches in circumference, and in form almost perfectly spherical. Though bruised by their fall, the pears remained sound till the beginning of December, when they became sweet and melting, though not at all highly flavoured: their flavour was, however, better than I expected, for they were blown from the tree long before they would have ceased to grow larger, if the state of the weather would have permitted; and the autumn of 1808 was so excessively wet, that some St. Germain pears, which grew on a south wall in the same garden, were wholly without richness or flavour.

The new pear. The new pear very much resembled the St. Germain in the form of the eye and stalk, and the almost perfectly spherical shape is that which might have been anticipated from the forms of its parents. It will probably acquire a very large size under favourable circumstances; but removing from my late residence at Elton, I have been under the necessity of again transplanting the tree, and therefore I cannot expect to see its fruit in any degree of perfection till the year 1811. Subsequent attempts. I have subsequently attempted to form other new varieties by introducing the pollen of the beurrée, crassane, and St. Germain pears, into the prepared blossoms of the autumn bergamot, the swan's egg, and Aston town pears; but I have not yet seen the result of the experiments. The leaves and habits of some of the young plants afford, however very favourable indications of the future produce.

The seeds In the preceding experiments I have always chosen to propagate from the seeds of such varieties as are sufficiently hardy

hardy to bear and ripen their fruit, even in unfavourable seasons and situations, without the protection of a wall; because in many experiments I have made with the view of ascertaining the comparative influence of the male and female parents on their offspring, I have observed in fruits, with few exceptions, a strong prevalence of the constitution and habits of the female parent; and consistently with this position the new pear I have described grew very freely in an unfavourable season, and in a climate in which the St. Germain pear, when its blossoms do not perish in the spring, will not grow at all, without the protection, and reflected heat, of a wall. I would therefore recommend every person, who is disposed to engage in the same pursuit, to employ the pollen only of such pears as the St. Germain, the d'Auche, the virgoleuse, the bezi, the chaumontel, the colmar, or bergamotte de pasques, and the seeds of the more hardy autumnal and winter kinds.

should be from hardy varieties,

I would also recommend the trees from which the seeds are to be taken, to be trained to a west wall in the warmer parts of England, and to a south wall in the colder, so that the fruit may attain a perfect, though late, maturity. Every necessary precaution must of course be taken to prevent the introduction of the pollen of any other variety, than that from which it is wished to propagate, into the prepared blossoms.

and these trained to a west or south wall.

I shall take this opportunity of pointing out to the Horticultural Society the merits of a new variety of plum, (Coe's golden drop) as a fruit for the dessert during winter, with which the public are not sufficiently well acquainted. Having suspended by their stalks, in a dry room, some fruit of this variety which had ripened on a west wall, in October, in the year 1808, it remained perfectly sound till the middle of December, when it was thought by my guests and myself, to be not at all inferior, either in richness or flavour, to the green gage, or drap d'or plum. I am informed by Mr. Whitley of Old Brompton, from whom I received it, that it bears well on standard trees.

A new plum.

XIII.

METEOROLOGICAL JOURNAL.

1812.	Wind	Pressure. Max.	Pressure. Min.	Pressure. Med.	Temperature. Max.	Temperature. Min.	Temperature. Med.	Evap.	Rain	
1st Mo.										
Jan. 6	N W	29·77	29·68	29·725	38	31	34·5	4		☽
7	N	30·05	29·68	29·865	38	33	35·5	—	·19	
8	N W	30·19	30·05	30·120	37	26	31·5	—		
9	N W	30·18	30·10	30·140	37	31	34·0	—		
10	Var.	30·18	30·13	30·155	36	32	34·0	—	·12	
11	N	30·13	29·89	30·010	38	32	35·0	—	·25	
12	N	29·89	29·79	29·840	42	32	37·0	—		
13	N W	29·98	2979	29·885	40	35	37·5	—	1	
14	N W	30·08	29·98	30·030	41	33	37·0	·14		●
15	W	30·18	30·03	30·130	45	28	36·5	—		
16	W	30·20	30·17	30·185	39	32	35·5	—		
17	N	30·25	30·20	30·225	38	31	34·5	—		
18	W	30·25	30·15	30·200	43	34	38·5	—	5	
19	S W	30·15	29·97	30·060	47	36	41·5	—	3	
20	N W	29·97	29·88	29·925	43	29	36·0	—		
21	N W	29·88	29·86	29·870	40	28	34·0	·33		☾
22	N W	29·96	29·86	29·910	41	31	36·0	—		
23	N E	30·08	29·96	30·020	34	31	32·5	—		
24	N W	30·10	30·06	30 080	39	27	33·0	—		
25	W	30·06	30·00	30·030	41	39	40·0	—	3	
26	S	30·07	30·05	30·060	45	31	38·0	—		
27	S W	30·05	29·87	29·960	47	31	39·0	·28		
28	Var.	29·87	29·46	29·665	46	36	41·0	—		○
29	S E	29 34	29·28	29·310	45	40	42·5	—	·12	
30	S	29·79	29·34	29·565	50	33	41·5	—	·24	
31	S E	29·79	29·79	29·790	48	41	44·5	·29	·10	
2d Mo.										
Feb. 1	S E	29·69	29·67	29·680	47.	42	44·5	—	4	
2	S E	29·64	29·34	29·490	50	40	45·0	—	8	
3	S E	29·69	29·45	29·545	47	42	44·5	—	2	
4	S W	29 58	29·45	29·515	49	42	45·5	·32	1	
				29·899			38·0	1·40	1·29	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES

NOTES.

First Month. 6. Very fine morning: wet evening: the night stormy with much snow. 7. Snowy morning, stormy day. 9. Snow fell through the night, to about three inches depth. 10. a. m. Little wind, changing to S. W.: a thaw. London was this day involved, for several hours, in palpable darkness. The shops, offices, &c. were necessarily lighted up; but, the streets not being lighted as at night, it required no small care in the passenger to find his way, and avoid accidents. The sky, where any light pervaded it, showed the aspect of bronze! Such is, occasionally, the effect of the accumulation of smoke between two opposite gentle currents, or by means of a misty calm. I am informed that the fuliginous cloud was visible, in this instance, from a distance of forty miles. Were it not for the extreme mobility of our atmosphere, this volcano of a hundred thousand mouths would, in winter, be scarcely habitable! 16. A dripping mist. 18. Misty morning. 19. Very cloudy: large lunar halo: stormy night. 22. Snowy evening. 23, 24. Lunar halo. 28. Windy night. 29. Windy morning: wet evening.

Second Month. 2. Gloomy, with small rain at intervals. About half past 7, p. m. the wind rose and blew furiously from E. and S. E. about an hour and a half, the Barom. falling a quarter of an inch: abating afterwards, it rose again, and the night was stormy.

RESULTS.

Winds from the N. and W. to the time of Full Moon, then from the Eastward.

Barometer: highest observation 30·25 inches; lowest 29·23 inches; Mean of the period 29·899 inches.

Thermometer: highest observation 50°; lowest 26°; Mean of the period 38°.

Evaporation 1·40 inches. Rain, (including the products of snow) 1·29 inches.

The observations on the Barometer for the period, and the greater part of the Notes, were made at Stratford, by my friend John Gibson.

LONDON, L. HOWARD.
Second Month, 22, 1812.

XIV.

XIV.

METEOROLOGICAL TABLE for the Year, 1811,

Extracted from the Register kept at Kinfauns Castle, the Residence of Lord GRAY, *Three Miles from Perth, N. Britain, for the Year* 1811. *Communicated by his Lordship.*

1811.	Morning, 8 o'clock Mean height of		Evening, 10 o'clock Mean height of		Tot. rain fallen.	Number of days.	
	Barom.	Ther.	Barom.	Ther.	Inches.	Rain or snow.	Fair.
January.	29·87	33·19	29·55	33·32	1·45	14	17
February.	29·40	34·83	29·46	34·70	2·63	16	12
March.	30·04	39·80	30·25	39·70	0·90	9	22
April.	29·77	42·90	29·78	40·30	1·91	14	16
May.	29·84	50·03	29·83	47·70	3·12	20	11
June.	29·89	54·60	29·91	51·80	2·20	18	12
July	29·99	58·01	30·00	55·50	2·86	14	17
August.	29·89	54·83	29·96	52·75	2·71	18	13
September	30·17	50·45	30·18	50·96	1·78	8	22
October.	29·77	49·54	29·78	49·59	4·41	25	6
November	29·96	42·50	29·98	42·45	2·97	14	16
December	29·78	35·03	29·80	34·85	1·80	15	16
Average of the year.	29·86	45·47	29·87	44·47	28·74	185	180

Kinfauns Castle is three miles almost due east from Perth. The house stands about ninety feet above the level of the River Tay, and probably only a few feet more above that of the sea.

Twickenham, 21 *Feb.* 1812.

*** The Author will be happy to receive an annual communication from his Lordship.

XV.

Remarks on some Electrical and Electrochemical Phenomena: by GEORGE JOHN SINGER, *Lecturer on Chemistry and Natural Philosophy.*

Electrical law of induction.

IN the last number of the Philosophical Journal, a correspondent, A. Z., concludes his remarks on Mr. Anderson's experiments

experiments by observing, "that the electrical law of *induc-* "*tion*, which Mr. Murray has pointed out," affords an explanation of the manner in which the decomposition, that occurs at every interruption of a metallic circuit in a fluid, is effected.

The term induced by Mr. Davy.

The term *induction* was I believe unknown in electrical science, till introduced by Mr. Davy. That excellent chemical philosopher has indeed employed it very extensively, and appears to consider it as expressing the most important principles of electrical action.

Much obscurity occasioned by it.

The *general* application of this term to so many important phenomena has been productive of much obscurity. Its meaning, according to Mr. Davy's application, has never been clearly defined, and is by no means obvious; while its promiscuous employment in the explanation of various, and diametrically *opposite* effects, is contradictory, absurd, and unintelligible.—My present leisure will not allow me to speak of this subject so fully as may be requisite to its proper elucidation; but to counteract erroneous impressions, I shall briefly state some of those inconsistencies, to which I have referred.

Mr. Davy uses it to express the action produced by approximation without contact.

Induction, as applied to *any* electrical phenomena, is unquestionably an objectionable term, as in its literal interpretation it expresses nothing analogous to any known electrical effect. By an attention to the writings and lectures of Mr. Davy, it may however be inferred, that his intention is to express that species of action, which results from the approximation, *without contact*, of an unelectrified to an electrified conductor; which has been called by Volta, and other electricians, electrical influence, and ascribed by Lord Stanhope to "the nature of an electrical equilibrium." It is well known to electricians, that an insulated conductor, when electrified either positively, or negatively, will alter the electrical state of any other body brought within a *certain* distance of it. This alteration will occur (though rather differently) whether the presented conductor be insulated or not; but it cannot be properly investigated, unless the conductor is insulated. As an example, the following well known but instructive experiment may suffice.

Experiment to

Insulate in a horizontal position a metal rod with blunt

or

illustrate this action.

or rounded terminations, attach to each extremity of this rod a pair of pith balls, let the rod and its appendages be represented by A—B. Present the extremity A to a positively electrified surface, but *not* within its striking distance. Both pairs of balls will open; and, if examined, A will be found negative; B, positive. Remove the rod from the vicinity of the electrified surface, the balls immediately collapse, and every electric sign ceases. A proof, that no electricity has been communicated to it, and that the electrical appearances produced arise only from the unequal distribution of the natural electricity of the rod, during its approximation to the electrified surface; seeing that, as soon as this *cause* is removed, the effect it has produced immediately disappears.

No electricity communicated.

Experiment in which the electricity is communicated.

If the preceding experiment be varied, by bringing any part of A—B *within* the striking distance of the positive surface, each pair of pith balls will open as before, but they will be *similarly* electrified, being *both positive*; and this effect (arising from *communicated* electricity) will be permanent; for, on removing the rod A—B from its proximity to the electrified surface, the divarication of its balls will continue.

The same term therefore should not be applied to express different effects.

There are some variations of this experiment very important to electrical theory, but I mention here only the most simple facts, as my intention is merely to show the impropriety of applying the same explanation to *contrary* and opposite effects.

An electrified surface may act in two ways:

From what has been stated it will be evident, there are *two* methods by which an electrified surface may excite electrical effects in other bodies.

by approximation,

1st. By approximation. In this case the electrified surface loses *none* of its intensity, and the previously unelectrified body becomes electrical only in consequence of the *unequal distribution* of its natural electricity.

and by communication.

2nd. By direct communication. In this case the original surface *loses* part of its intensity, and the body previously unelectrified becomes electrical in consequence of an *alteration* in the *quantity* of its natural electricity.

The different effects of these two methods of electrification are

A.

A. By the first, a positive surface may be employed to electrify another body positively, or negatively; or it will electrify the same body at *once* positively, negatively, and neutral in different parts of its length; but in this latter case, the electrical effects will last only *during the approximation.*

Different effects of these.

B. It is not possible to produce these effects of approximation, unless the bodies be *separated* by a *nonconductor*, the *resistance* of which is sufficient to prevent the passage of electricity from one to the other.

C. By the second method a positive surface can only communicate positive electricity; and *vice versa*. But these *communicated* effects are *permanent* after the *separation* of the bodies.

D. The effects of communicated electricity can only take place by the *actual contact* of conducting bodies, or where the intervening medium does *not completely resist* the passage of electricity.

No two series of phenomena can I think be more distinct than the preceding; their conditions of action are directly contrary, and it must be consequently obvious, the same explanation cannot apply to both.

These two sets of phenomena distinct.

Mr. Davy has however for a long time applied the term *induction* to some of these opposite cases of electrical action; for instance, the Leyden jar, and the insulated rod; distinct cases of approximation: the spiral tube, luminous word, electric spark, &c., decisive instances of communicated electricity.

Mr. Davy has applied the term induction to both,

In the Bakerian lecture for 1807, an explanation of the voltaic battery was attempted on similar principles; and this explanation has been repeated every succeeding season in the lectures at the Royal Institution. It assumes, "that, with regard to electricity of such low intensity, "water is an *insulating* body." An assumption, which has been decisively contradicted by the experiments of Mr de Luc, which prove, that the battery is a *conducting* column. Yet Mr. Davy continues to speak of *induction*, and electrical polarity; and A. Z. appears to think it capable of explaining the interrupted circuit; where the whole column is indisputably a *conductor*, and where the phenomena depend on

and attempted to explain the action of the pile on the same principle.

on the *circulation* of electricity *through its whole length!* or in other words, on the transmission of electricity from *wire* to *wire* through *water*.

The existence of a positive and negative point at each interruption of the circuit not proved.

I have already stated, that the chemical changes produced in fluids by voltaic electricity, at every interruption of the metallic circuit, are no proofs of the existence of a positive and a negative point at each of those interruptions; so long as we have *no evidence*, that the *chemical effects* are produced by *opposite* electrical states, and not by the peculiar modifications of a current. We have no such proof; nor have we yet electrometrical demonstration, that the opposite extremities of every wire in an interrupted voltaic circuit are oppositely electrified, though Mr. Davy has very recently said "it would be easy to show this." Mr. de Luc's analysis* is the only instance of an accurate examination of the electrical state of the wires, compared with their chemical effects; his conclusions are however strikingly opposed to those of electrical energy; they have been long published, and are not yet controverted.

Mr. de Luc's analysis.

Let us suppose however for a moment, that the opposite electrical states are essential to the chemical effects. Can we have recourse to *induction* to explain them? Certainly not, unless by *induction* we mean communicated electricity, circulation of electricity, or current of electricity; for these alone express what we see and demonstrate in the experiment; and are indeed employed (certainly incautiously) by the same philosophers who speak of electric energy.

Induction an improper term.

If induction be strained to express these terms, it cannot possibly be applied to the opposite series of phenomena, viz. those produced by approximation only; where no current is produced, but merely a temporary disturbance of the electrical equilibrium effected.

Hence I think it fair to conclude, that induction, far from an electrical law, is a term ill suited to express any electrical action; a term which it would be contradictory and absurd, to apply to all the varieties of this influence; and which is in any case objectionable, as involving the assumption of a principle of action not proved to exist.

* See Journal, vol. XXVI, pp. 113 and 241.

The

The fallacy of this principle in one of its most popular applications, the hypothesis of the Voltaic battery, I shall consider in a future letter.

3, *Prince's Street, Cavendish Square*,
February 15th, 1812.

XVI.

On some new *Varieties of the Peach*. *By* T. A. KNIGHT, *Esq., F. R. S., &c**.

IN the Transactions of the Horticultural Society of 1807, I have mentioned some experiments I had made with the hope of obtaining new and early varieties of the peach†, which might prove better calculated for our climate, than those which have been imported from the southern parts of Europe: and as the character of some of the plants that I have raised affords a fair prospect of success, I have thought the following account sufficiently interesting, to induce me to send it to the Horticultural Society.

Experiments for obtaining new varieties of the peach.

In efforts to obtain new varieties of fruits of other genera, I have had reason to conclude from the success of former experiments, that the trees, from blossoms and seeds of which it is proposed to propagate, should have grown at least two years in mould of the best quality; that during this period, they ought not to be suffered to exhaust themselves, by bearing any considerable crop of fruit; and that the wood of the preceding year should be thoroughly ripened, (by artificial heat when necessary) at an early period in the autumn: and if early maturity in the fruit of the new seedling plant is required, I think, that the fruit, within which the seed grows, should be made to acquire maturity within as short a period as is consistent with its attaining its full size, and perfect flavour: those qualities ought also to be sought in the parent fruits, which are desired in the offspring; and the most perfect and vigorous

Management of the trees for blossoms and seeds.

* Trans. of the Hort. Soc. vol. I, p. 165.

† See Journal, vol. XVIII, p. 195.

offspring

offspring will be obtained, of plants as of animals, when the male and female parent are not closely related to each other*.

Experiment.

The varieties of the peach, from which I first propagated, were the large French mignon, and the little red nutmeg, using the stigmata of the former, and the pollen only of the latter. The trees of each variety had been removed early in the spring of the preceding year (1801) from pots of moderate size into others which were very large, and were filled with mould of the most favourable quality, that I could compose; and in these pots the plants had grown with excessive vigour. The aid of artificial heat was employed in the spring of 1802, to enable the wood and blossoms of each plant to acquire the most perfect state of maturity in the succeeding autumn; and during winter the pots were defended from severe frost, that the minute fibrous roots of the plants might be wholly preserved; and as the spring approached the trees were kept in as low and equal a temperature as possible, that the powers of life, in the plants, might not be prematurely excited into action, or in any degree uselessly expended. Nevertheless, owing to the wood and buds having acquired maturity early in the preceding autumn, and an accumulated excitability from long rest and cold, the blossoms began to swell rapidly on the first approach of spring; and very early in March it became necessary to place the trees in the forcing-house, the blossoms being so far advanced, as to be subject to some danger from frost.

As soon as the blossoms had fallen, the fruit was ripened under every advantage of heat and light, that I could command, the glass having been taken off every favourable hour, during the last swelling of the fruit, to admit the solar rays, without its intervention. Three French mignon peaches only were suffered to remain on each tree, and six of these, (which attained the greatest state of perfection), afforded me eight plants in the succeeding spring. The plants were two years old when mentioned in a former

The trees bore at three years,

* See Horticult. Trans. of 1807, Part. I, p. 30, or Journ vol. XVIII, p. 189.

communi-

communication, and I then inferred, from the rapid change observable in the character of the leaves and general growth, that they would bear fruit, as they subsequently did, when three years old.

Of the new varieties thus obtained three are very early; but I have not had an opportunity of comparing their time of ripening with that of the earliest old varieties. For the red nutmeg peach did not succeed at all in my garden, and the blossoms of the early Anne were wholly destroyed by the unfavourable weather of the spring of 1807 and the following year. Two of the new varieties, however, ripened ten days before the royal George peach, and three weeks before the red Roman nectarine, which grew on the same wall, and adjoined the seedling trees; and therefore I conceive these not to be much later varieties than their male parent, which they strongly resemble in colour, and in the form and character of their leaves: but their fruit is much larger, many having exceeded $7\frac{1}{2}$ inches in circumference. The fruit of each of the new varieties is soft and melting, and very readily quits the stone; and I thought the flavour of one of them quite equal to that of any peach which my garden produced. In their leaves and fruit, every tree forms a perfectly distinct variety, and even where the same stone contained two plants, they bear very little resemblance to each other.

and the fruit ripened early.

Each tree a distinct variety.

In the present spring I exposed all the seedling plants without any covering, to ascertain the comparative degrees of hardiness of their blossoms; and in this respect I found them to differ very widely. The blossoms of two of the varieties appear, however, to be very hardy, and promise an abundant crop of fruit, though the season has been more than usually unfavourable; and I have had the pleasure to observe, that the best peach is one of the most hardy.

Different in hardiness.

The success therefore of the first and of the only experiment, of which I have fully seen the result, on this species of fruit, has fully answered, and indeed exceeded my hopes; and I entertain little doubt that the peach-tree might, in successive generations, be so far hardened and naturalized to the climate of England and Ireland, as to succeed well as a standard in favourable situations. It is my wish to try the

Hope of obtaining standard peach trees.

Experiments intended.

the effects of propagating successive generations alternately from the open wall, and from the hot-house, and of introducing the pollen from the open wall to the blossoms of the hot-house, with the hope of obtaining varieties which will be at once hardy and early. The peach does not, like many other species of fruit, much exercise the patience of the gardener, who raises it from the seed; for it may always be made to bear when three years old, and there is something in its habits which induces me to believe, that it might be made to bear at two years old. I will not venture to decide, whether it might not possibly produce fruit even at the end of a single year; and therefore, as the improvement of this, and other species of fruit, and adapting varieties of them to our climate, presents an ample and interesting field for experiment, I trust that I shall not labour in it alone.

The peach bears soon.

Cautions.

In prosecuting such experiments, I would recommend the seedling peach trees to be retained in pots, and buds from them, only, to be inserted in older trees; for their rapid and luxuriant growth is extremely troublesome on the wall, and pruning is death to them.

XVII.

On the Areolites, that fell near Lissa in Bohemia, on the 3d of September, 1808: *by Mr.* REUSS, *Counsellor of Mines*.*

Account of some meteoric stones that fell near Lissa in Bohemia.

THE account we have of this fall of stones was collected on the spot, by the mayor of the place, four days after it happened. Farther information was collected afterward by Mr. Merkl, counsellor of state, who has lodged an official statement of it in the chancery.

Lissa is a small town of the circle of Buntzlau, four miles W. N. W. of Prague, and as many S. S. W. of Jungbuntzlau; two miles N. of Benatek; two E. of Altbuntzlau and Brandeis; and two W. of Nimbourg.

* Ann. de Chim. vol. LXXIV, p. 84. Abridged by Mr. Tassaert.

The

The country where these stones fell is a plain, extending southward to the Elbe. The soil is a poor dry sand, fit only for rye; and the rocks that are found there are of an argillaceous gritstone impregnated with iron. The field on which one of the stones fell was a very loose sand, that had just been ploughed up; yet the stone penetrated into it only four inches. The second fell in an adjoining field, the soil of which was rather harder, and more clayey; yet it penetrated only four or five inches. The third fell in a small wood of fir trees, on a sandy ground covered here and there with turf, and likewise made an impression four or five inches deep. The fourth, which was found about two thousand paces from the village of Straton, weighed two pounds and a quarter; but one corner was broken off, without the inquirer's being able to learn how. The third stone, that fell in the wood, weighed five pounds nine ounces and half, though all its angles and edges had been damaged. The direction in which these stones fell was from the north.

The place described.

The circumstances, that accompanied this fall of stones, were nearly the same, as have been observed in other places. On Saturday the 3d of September, 1808, at half after three o'clock in the afternoon, a loud explosion was heard, which all the witnesses compared to a discharge of several pieces of cannon, succeeded by a noise like that of firing by companies, or the roll of drums. This noise continued a full quarter, or even near half an hour. The sky, which had been very clear, appeared covered as with a slight gauze; yet the rays of the Sun penetrated easily through this sort of thin mist. The night preceding had been fine, calm, and very clear; the weather had been fine the whole day, except about noon, when a few drops of rain fell, but the clouds dispersed; and about three o'clock in the afternoon the heat was considerable, and the weather heavy *(lourd)*.

Circumstances accompanying the fall.

No person saw these stones fall, so that we know not whether they were black, or red, or smoking, when they fell; but some reapers, who took up one as soon as it had fallen, found it as cold as the stones around. It did not soil the fingers, and none of them had any smell of sulphur. No person observed any lightning, or luminous meteor; neither

rain nor wind was noticed; and no one felt any of that uneasiness or oppression, that indicates electricity.

The stones described. These aerolites, like all others, were of a mixed substance. They are of a light ashen gray colour, fine grained, traversed in all directions by little veins, and interspersed with little disseminated globules. Their specific gravity is 3·56. Brought near a compass, they cause the needle to move through an arc of 8°. When reduced to powder, globules may be extracted from it by the magnet.

Analysis of the aerolite of Lissa, by Mr. KLAPROTH.

Analysis of the stone. Though all the external characters of this stone of Lissa lead to the presumption, that it must contain the same substances as those, which chemical analysis has demonstrated in other meteoric stones; yet the subject is too interesting, to allow us to neglect an accurate examination of every fresh specimen, for the purpose of discovering how or in what proportions it may differ from those already analysed.

Mr. Reuss having sent me a sufficient quantity of this stone, I subjected it to the following analysis.

Iron extracted by the magnet. *a.* 200 grains were reduced to powder; and from these 29 grs were extracted by the magnet. These were in small ramified particles. The remaining powder still contained some small shining metallic points, which might be considered as sulphuret of iron, as sulphuretted hidrogen gas was obtained from it on treating it with muriatic acid.

The metal dissolved in muriatic acid. *b.* The 29 grs of metal were dissolved in muriatic acid by the assistance of a gentle heat. Sulphuretted hidrogen was evolved, and the liquid at first appeared foul and milky. Five grains of the powder of the stone, that had adhered to the globules of the iron, remained undissolved. The acid liquor had not the emerald green colour, that the solution of meteoric iron commonly has; but was simply greenish, which indicated but a small portion of nickel. In order to oxide the iron completely, I added nitric acid to the boiling liquor, precipitated the oxide of iron by ammonia, and filtered. The ammoniacal liquor was of a pale blue colour. On evaporating it to dryness, and heating it red hot in a platina crucible, a little yellowish gray residuum was left. This residuum, dissolved in nitric acid, formed a green liquor,

liquor, which became blue on supersaturation with ammonia. This liquor, evaporated anew, yielded an apple-green salt, which was heated red hot, to decompose the nitrate of ammonia. The residuum, which was black, was again redissolved in nitric acid, and filtered, to separate a blackish matter acquired from the platina crucible. The nitric solution, precipitated by carbonate of soda, yielded a pale green carbonate of nickel.

c. The 171 grs of stony powder left by the magnet in experiment *a*, with the five grains of earthy residuum, were heated in a silver crucible with twice their weight of potash. This mixture became blueish by fusion. Diluted with water, the lixivium assumed a greenish hue. The filtered alkaline liquor remained clear when neutralized by nitric acid. The solution was evaporated to dryness, the salt redissolved in water without leaving any residuum, and on adding nitrate of mercury nothing but a white precipitate was obtained. This trial, which had been instituted for the detection of chrome, did not afford the slightest indication of this metal; though some have asserted, that it exists in aerolites.

The stony matter treated with potash, lixiviated,

d. The powder of the stone having been well lixiviated, and treated with muriatic acid, dissolved in it by the assistance of heat. The liquor was evaporated to dryness, and the residuum redissolved in water and filtered. The silex, well washed and heated redhot, weighed 83·5 grs.

and treated with muriatic acid.

e. The muriatic solution, freed from silex, was precipitated cold by carbonate of potash. The alkaline liquor, separated from the brown precipitate, was subjected to ebullition, and mixed with as much carbonate of potash, as was requisite to precipitate it. The precipitate consisted of carbonate of magnesia.

The solution precipitated by carbonate of potash.

f. The brown precipitate formed by the carbonate of potash in experiment *e* was boiled with caustic potash. The alkaline liquor, supersaturated with muriatic acid, and then precipitated by carbonate of potash, yielded a white flocculent precipitate; which, after being heated redhot, weighed 2·5 grs; and was found to be alumine, on treating it with sulphuric acid.

The precipitate treated with caustic potash.

g. The brown precipitate, which had been treated with potash, was dissolved in nitric acid; and, after the too great excess of acid had been saturated by soda, the liquor was precipi-

Dissolved in nitric acid, and precipitated by succinate of iron.

precipitated by succinate of iron, and the precipitate heated redhot: then, after adding the oxide of iron obtained in experiment *b*, and dropping on it a little oil, it was heated redhot in a close vessel. The oxidulated iron in this state weighed 80 grs, answering to 58 of metallic iron.

The liquor precipitated,

h. The liquor separated from the iron was precipitated by carbonate of potash while boiling. A greenish white precipitate of carbonate of magnesia was obtained, which was added to the carbonate of magnesia of experiment *e*, and exposed to a strong red heat. This changed the colour reddish, and the magnesia weighed 48 grs. Being treated with sulphuric acid diluted with water, half a grain of oxide of manganese was separated from it.

and the precipitate treated with sulphuric acid.

The solution evaporated and redissolved.

i. The sulphuric solution was evaporated to dryness, and the salt redissolved in a great deal of water. Some silex separated, which, after calcination, weighed 2.5 grs. On evaporating the solution, small acicular crystals were obtained, which were sulphate of lime, and weighed 3 grains, equivalent to one grain of lime.

k. The remainder of the solution afforded nothing but sulphate of magnesia, the quantity of base in which was only 44 grains, when the weight of the silex, lime, and oxide of manganese was deducted. The colour of the sulphate of magnesia still tending to green, the presence of a little nickel was to be presumed. Accordingly the salt was redissolved in water, and the nickel precipitated by a stream of sulphuretted hidrogen. The oxide thus obtained was mixed with that of experiment *b*, and exposed to a strong red heat; after which it weighed 1·5 gr., answering to 1 gr. of nickel.

From the results of this analysis it appears, that 100 parts of the aerolite of Lissa gave

Component parts of the stone.

Iron	*g*		29
Nickel	*k*		0·50
Manganese	*h*		0·25
Silex	*d* 41.75 *i* 1·25		43
Magnesia	*k*		22
Alumine	*f*		1·25
Lime	*i*		0·50
Sulphur and loss			3·50
			100

I have

I have supposed, that all the iron in the aerolite was in the metallic state. Formerly such as could be extracted by the magnet was alone so reckoned, the rest being considered as oxide of iron. But as there is no sign of oxidation in this aerolite recently fallen, it is evident, that the shining points, which did not adhere to the magnet, were pyrites, in which the iron was contained originally in the metallic state.

Proust's hypothesis.

The hypothesis of Proust, that aerolites are products of our globe, expelled from the polar regions to fall nearer the equator, is founded on the total absence of oxigen.

That of others.

This circumstance, however, is equally favourable to the opinion of those, who suppose them to be thrown from the moon; since astronomers deny to this satellite an atmospher containing oxigen, and saturated with watery vapours like that of our globe.

The stone could not have been formed in the atmosphere.

But it is certain, that the total absence of this principle completely refutes the opinion of those, who believe, that these aerolites are formed in the regions of our atmosphere; since the particles of iron and martial pyrites would not remain even so short a time without a commencement of oxidation.

This analysis of an aerolite so recent affords a fresh proof, that they are all nearly of the same nature; as the preceding account by Mr. Reuss shows, that they have all been projected from higher regions. But the naturalist, who would honestly build only on certain facts, must not be ashamed to confess, that he is ignorant of their origin.

Meteoric stone of Stannern.

I shall add, that a powdered specimen of an aerolite, which fell near Stannern, in Moravia, on the 22d of May, 1808, and of which consequently I know not the external characters, has been sent me. This stone would be a striking exception to all the aerolites known, since, from my analysis of a very small quantity, it would appear to be a decomposed basaltes. It is to be wished therefore, that the analysis should be repeated with a piece of the stone in its entire state, possessing all the characters necessary to prevent suspicion respecting it*.

* This stone has been analysed by Vauquelin, see Journal, vol. XXV, p. 54; and no doubt the pretended specimen sent to Klaproth in powder was an imposition. C.

XVIII.

XVIII.

An Answer to the Observations of Dr. PEARSON, *(see our last Number) on certain Statements respecting the Alkaline Matter contained in Dropsical Fluids, and in the Serum of the Blood. By* ALEXANDER MARCET, *M. D. F. R. S., one of the Physicians to Guy's Hospital.*

To Mr. NICHOLSON.

SIR,

The question not placed in its proper light.

ALTHOUGH I feel extremely disinclined to engage in any public philosophical controversy, especially when the object is to vindicate statements, the truth of which any common observer may easily ascertain by experiment; yet, as there are some points in the above communication, which do not place the question in its proper light, and might mislead those, who have not the opportunity of referring to the original documents, I have thought it necessary, to offer in return a few observations.

Its proper state.

The state of the question is simply this: All chemists have for a long time agreed that the blood, and probably all the animal fluids, contain, together with various neutral salts, a certain portion of alkali not combined with any acid. This alkali has generally been considered as being soda, although a few chemists had also noticed traces of potash in some of these fluids. Dr. Pearson, on the contrary, in examining various kinds of animal substances, and especially of expectorated matter, was led to conclude that the whole of the uncombined alkali contained in the animal fluids was potash, and that they did not contain uncombined* soda in any proportion whatever.

Soda the only uncombined alkali in the fluids.

In analysing the fluids of dropsy, I was naturally led to inquire into this question; and the result obtained induced me to conclude, that the only uncombined alkali present in the blood, or other animal fluids, was soda; and that the indications of potash, which, by applying the test used by Dr. Pearson, I was able to detect in these fluids, were

* By the expression *uncombined*, I mean not combined with acid.

owing

owing to the presence of this alkali in a state of combination with the muriatic acid.

This proved in two ways.

The experiments I adduced in evidence were of two kinds; some of them showing that the uncombined alkali was soda, and others that it was not potash.

Salts obtained from different animal fluids.

Portions of saline matter being procured from various animal fluids by evaporation and incineration; and brought by subsequent redissolution and evaporation to a crystalline state, crystals of determinate forms were obtained, some of which appeared to consist exclusively of sub-carbonat of soda, some of muriat of soda, and others of muriat of potash; but none could be detected, which appeared to contain any carbonat of potash.

The uncombined alkali treated with acetic acid and alcohol.

Other similar portions of the saline matter being treated with acetic acid, in order to bring any uncombined alkali present to the state of acetat; and alcohol being added with a view to separate these acetats, the residue of this alcoholic solution appeared to consist almost* solely of acetat of soda; while, on the other hand, potash was found in the residue left undissolved by the alcohol.

Potash in a state of combination.

In these various trials the presence of potash, in a state of combination, was proved by the tests of oximuriate of platina and tartaric acid, both of which form precipitates with potash, and not with soda.

The uncombined alkali soda.

The uncombined alkali, on the contrary, was shown *not* to be potash by the last-mentioned tests failing to indicate the presence of this alkali; while, on the other hand, it was proved to be soda, by the action of nitric acid, which, in combining with it, formed crystals of a rhomboidal, instead of a prismatic figure.

Dr. Pearson's objections examined.

I shall not enter into the particulars of these operations, because they are minutely related in the communication which has given rise to this discussion; but I shall now rapidly examine the principal objections which Dr. Pearson has made to the above conclusions.

1st objection.

Dr. Pearson's first ground of complaint is, that, instead of showing his conclusions to have been erroneous; that is,

* A trace of potash was detected in the alcoholic solution; but it must be remembered, that alcohol, however rectified, will take up minute portions of muriat of potash, or indeed of almost any other soluble salt.

I con-

I conceive, instead of following him step by step in his inquiry, I have contented myself with exhibiting my own experiments and conclusions. But I beg to observe, that the object of my inquiry was not to repeat Dr. Pearson's experiments, but to examine dropsical fluids; and that, if in the course of my analysis I met with results which militated against his conclusions, it could not be reasonably expected, that, in stating these results, I should think it incumbent upon me to wade through his laborious researches on the various forms of sputum or expectorated matter. I might indeed have abstained altogether from referring to his labours; but I thought it due to him, as a philosophical inquirer long known in the chemical world, to point out such similarities or discordances of results, as occurred in our respective experiments; thus referring the matter to the decision of physiologists, and showing, that there was no wish on my side to overlook the authority of former inquirers.

In endeavouring to analyse the various objections brought forward by Dr. Pearson, I am so often at a loss to understand his meaning; and I must add, so much embarrassed by the obscure and inaccurate manner in which he has stated some of my own proceedings, that it would be a task equally fruitless and laborious to follow his steps closely. I must, therefore, as much as possible, select those objections which are of a specific nature, and may be answered by an appeal to experimental evidence. Such is, for instance, the argument which he employs, no less than three times, (once in support of his own experiments, and twice with a view to invalidate my inferences), on the effects of alcohol and acetic acid, which argument is founded upon his belief, that acetat of soda is *not* soluble in alcohol, and that it is *not* a deliquescent salt; two palpable errors, which half a grain of this salt, and a few drops of alcohol, with no other apparatus than a watch glass, would have enabled him to rectify.

Argument from the effects of alcohol and acetic acid:

But the objection, which recurs the most frequently, and that upon which the greatest stress is laid, is the minuteness of the quantities of saline matter subjected to experiment. It would appear, that Dr. Pearson questions whether

from the minute quantity of saline matter used.

whether a few grains of saline matter may be expected to yield results similar to those which would be obtained from larger quantities; whether, for instance, the same inferences might be drawn from rhomboidal crystals of a minute size, as from similar crystals of larger dimensions; or whether experiments tried upon an ounce or two of my dropsical fluids, may be brought into competition with those which he performed upon two or three pints of his ropy sputum.

Such a scepticism, I must own, I have myself never entertained. I have always thought on the contrary, that the chemical properties, which belonged to a particle of matter, were exactly similar to those, which would be found to belong to a whole mountain of the same substance; that a rhomb of only one hundredth part of an inch might be characterised by its form as distinctly as one a hundred times larger*. But I carry the point still farther, for I go the length of believing, that many experiments of research may be wonderfully facilitated by analysing upon a small scale; that a great deal of convenience, of economy, and sometimes even of accuracy, may thus be gained; and that in some instances we may even obtain new and unexpected powers of inquiry by operating upon small quantities†. Experiments on a small scale vindicated.

Thus, were it not for the assistance of minute or micros- Instances of

* Thus I have no hesitation in maintaining, that unless it be proved, that nitrate of potash may crystallize in rhombs, my conclusions respecting the particular point in question, would stand upon that evidence alone; or that unless it be shown, that carbonate of potash may crystalize in cubes, my inference respecting the presence of muriate of potash stands uncontroverted.

With regard to my attempt at expressing centesimal parts of grains, which is, with some apparent reason, noticed as an instance of singular pretention to accuracy, I beg to observe, that I have never actually attempted to weigh smaller quantities than decimal parts of grains; and whenever smaller fractions have been expressed, they have arisen from a conversion of those numbers to some general standard.

† I would also observe, while upon this subject, that there is a degree of neatness gained, by reducing the scale of operations, which is often incompatible with processes in the large way. Thus I have never found it necessary, in analysing, to introduce among the enumeration of contents, "a little dirt," as some old-school chemists have been in the habit of doing.

copic

their aid to science.

copic observation, a great number of important facts, which have enriched chemistry within the last 20 years, would, in all probability, have remained undiscovered; and this country might not have obtained that first rank in philosophical chemistry, to which it has but lately been raised, and which it had long held in other departments of science.

Is it necessary that I should specify particular instances? Can any philosopher, attentive to the progress of analytical chemistry, overlook so many discoveries, in which neither furnace nor forge, nor subterraneous laboratories have been concerned, in which a watch glass, a blow pipe, and a few drops of chemical reagents, have been all the instruments required? Were not, for instance, the analyses of the Iceland springs, by Dr. Black, (the same eminent philosopher to whom Dr. Pearson appeals, as an authority against microscopic observations), performed upon quantities of saline matter, of astonishing minuteness? Surely Dr. Pearson cannot have forgotten, that it was by the accurate examination of only a few grains of matter, that the nature of no less than five kinds of urinary calculi has been ascertained, and their discrimination rendered easy and certain; that the nature of diamond has been established; that no less than four new metals have been discovered in the crude ore of platina; that the similarity between all the meteoric stones has been proved; that the identity of the chemical agencies of electricity, whether excited by the common machine, or by the voltaic battery, has been demonstrated; that in a neighbouring country the formation of crystals has been explained upon systematic principles; that among us a new and wonderfully accurate instrument of crystallography has been invented; and above all, that the metallic bases of alkalies, those extraordinary bodies which Nature had hitherto concealed under an impenetrable disguise, have at last been brought to light. Let it be remembered as one of the most glorious circumstances of that discovery, that it was by examining mere atoms of these substances, that their properties were first ascertained; and that when, in consequence of subsequent improvements in the mode of obtaining these bodies, they were procured in larger quantities, and their general properties were reexamined,

no

no error was discovered, and no important information was added to that which had originally been gained from microscopic quantities.

It is far from my intention, however, to contend, that on some occasions, new and important facts may not be brought to light by means of processes conducted upon an extensive scale, which would not admit of being reduced to a small compass. I only mean to assert, that such instances are comparatively but rare; and that no philippic against the examination of small objects; no appeal to old Masters; no slight upon modern improvements, ought to deter chemical inquirers from adopting methods, which some of our contemporaries have employed with so much utility and success.

Experiments on a large scale not without their use.

Among other inaccuracies in the critique which has given rise to these remarks, my paper on dropsical fluids has been represented as being the joint work of Dr. Wollaston and myself; for which supposition there was no other authority but a note in the paper in question, in which I acknowledged my obligations to Dr. Wollaston for the information and assistance, which I have, on this and other occasions, derived from his kindness. I need not say how highly I should have been flattered by such an association; but I think it due to him to state, not only that he had no share in the general inquiry, but that he did not even see the paper in question previous to its publication.

Dr. Wollaston inaccurately joined with the author.

I cannot refrain from noticing, among Dr. Pearson's remarks, another kind of licence, which appears to me still less warrantable. I allude to the practice of quoting in italics or placing between inverted commas, words or phrases which have not been used, and to seize upon them as a subject of ridicule. This is the case with some proposed *elegant* changes, and with my supposed recommendation to transfer chemistry to the "fireside of the drawingroom"; expressions which I have not used, and yet upon which Dr. Pearson has thought proper to be extremely jocular.

Expressions not used by the author cited so as to be taken for his.

I have not only farther to add, that should Dr. Pearson again write upon the subject, I shall not easily be induced to resume the controversy. I am sorry, therefore, to find it intimated at the conclusion of his paper, that he proposes

to

to continue his observations in your next number; and as it appears, that these intended remarks are meant as a return for the notice which I have taken of his papers, I regret the more that he should take so much trouble. For praise, when used as the vehicle of irony, is the worst kind of censure. The discovery of truth ought to be the only object of philosophical discussion. There are, doubtless many errors in my humble attempts at chemical analysis; but unless Dr. Pearson points out those errors, or brings forward new facts connected with my inquiries, I confess I had much rather he would not again honour them with his notice.

Truth should be the sole object of philosophical discussion.

"Quicquid id est, timeo Danaos, & dona ferentes."

I remain, Sir, &c.

ALEXANDER MARCET.

XIX.

On the supposed Presence of Water in Muriatic Acid Gas. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Experiment to show, that muriatic acid gas contains no water.

HAVING seen in your Journal for the last month a statement of Mr. Murray's relative to the presence of water in muriatic acid gas, and being present at a lecture of Mr. Davy's at the Royal Institution on the 7th of February, in the course of which he repeated it with very different results, I was induced to repeat it also. The mode of Mr. Davy's experiment was so very unexceptionable, I determined to adopt it; it was as follows, viz.: having obtained ammoniacal and muriatic acid gasses pure, I introduced them into a retort, which was previously exhausted. They immediately combined and formed muriate of ammonia. Then, having cleared a part of the neck, for any condensation of fluid that might occur, I applied heat, until all the salt was sublimed into the neck of the retort, and did not obtain a particle of moisture. I then removed some of the salt through the atmosphere into a dry tube, and applied

plied heat, and obtained vapour. I repeated this again, having suffered the salt to be exposed for a few minutes to the atmosphere, and obtained water again, so that Mr. Murray might have obtained in this way to thrice the weight of the salt he employed. Now in my opinion this clearly proves, that the water which Mr. Murray obtained, was from the atmosphere, and not from either of the gasses, as he thinks. It will be unnecessary to offer any observations on an experiment evidently so inaccurate.

I am, Sir, your most obedient,

A. B. C.

SCIENTIFIC NEWS.

Caledonian Horticultural Society.

As the improvement of horticulture is an object of considerable importance to the comforts of life and its innocent enjoyments, we insert the following summary of the series of prizes proposed by this society for the present year, as well as those of more general scope. Prizes proposed by the Caledonian Horticultural Society.

They are, under the first head, the silver medal, for the best early cucumber, grapes, spring brocoli, Brussels sprouts, winter lettuces, seedling polyanthuses, and early melon, to be shown on the second Tuesday of March or May: the best melon, forced peaches, cauliflowers, on the second Tuesday of June: the best seedling pinks, on the second Tuesday of July: the best twelve sorts of gooseberries, on the first Tuesday of August: the best peaches and nectarines from the open air, apricots, green gage plums, jargonelle pears, seedling carnations, and home-made wine without any foreign materials but sugar, on the second Tuesday of September: the best six kinds of apples and of pears, heads of late brocoli, forced sea-cale, and forced asparagus on the second Tuesday of December.

II. The production of *new* or *improved* varieties of fruits, culinary vegetables, or flowers.

1. For the best new apple, adapted to the climate of Scotland, raised from seed. Ten years to be allowed. Gold medal

Prizes proposed by the Caledonian Horticultural Society.

medal and twenty guineas. 2. For the best new pear, raised as above. Ten years to be allowed. Gold medal and twenty guineas. 3. For the best new peach or nectarine, raised as above. Six years to be allowed. Gold medal and ten guineas. 4. For an improved variety of the Dutch currant, raised from seed. Five years to be allowed. Gold medal. 5. For the best new and productive early melon. Gold medal. 6. For the best new early cucumber. Gold medal. 7. For the best new strawberry, raised from seed. Four years to be allowed. Gold medal. 8. For the best new sort of early potato, *without blossoms*, raised from seed. Five years to be allowed. Gold medal.

III. Communications, &c. (The gold or the silver medal to be awarded by the committee, according to the value and importance of the communication.)

[It is expected, that all communications will be founded on *actual* experiments.]

1. On the best method of improving the sorts of brocoli already cultivated, and of saving their seeds genuine in this climate. 2. The best method of cultivating and of forcing sea-cale. 3. The best treatise on orchard fruits adapted to the climate of Scotland, with lists and descriptions of the different kinds,—their habits of growth, &c.—their synonyms or local names; those for the table, and those for kitchen use. 4. The best treatise on the culture of the Dutch currant for wine. 5. The best mode of preventing or curing the mildew upon different fruit-trees and other vegetables. 6. The best mode of preventing or curing the canker in fruit-trees, &c. 7. The cheapest and most effectual mode of preserving fruit trees on walls from the effects of late spring frosts. 8. The best mode of destroying the blue insect, breeding in the crevices of the bark of apple-trees, and causing them to canker and die, chiefly on those trees imported from the London nurseries. 9. The best method of destroying wasps, woodlice, earwigs, &c., infesting wall-fruits. 10. The best mode of preventing the depredations of the turnip-fly. 11. The best method of preventing worms in carrot, in cauliflower, and brocoli roots. 12. The best mode of destroying the wire-worm. 13.

13. The best mode of destroying the pine bug, the brown scale, the white bug, the aphis or green fly, the chermes, the red spider, the thrips; or any other insect infesting hot-houses, pits, melon and cucumber frames, &c. 14. The best means of increasing the quantity of manure, and the best mode of applying it to different crops. 15. The best means of bringing into a bearing state full grown fruit-trees (especially some of the finest sorts of French pears) which, though apparently in a very healthy and luxuriant condition, are yet in a state of almost total barrenness. 16. The best account of a Scotch Kitchen Garden, or of a Scotch orchard. 17. The best method of preparing opium in this country; and the most advantageous manner of cultivating the white poppy for this purpose. 18. For the best essay on preventing the curl in potato. 19. For the best essay on destroying or preventing caterpillars on gooseberry bushes and fruit trees. 20. For the greatest quantity of asparagus, planted upon sandy land near the sea, and manured with sea weed only; not less than a quarter of an English acre. 21. For the greatest quantity of sea-cale, planted on the same kind of land, and manured with sea weed; not less than ten falls English measure. 22. For the greatest number of pints of strawberries produced from the smallest extent of ground, not less than a quarter of an acre.

Prizes proposed by the Caledonian Horticultural Society.

It is requested, that each article brought in competition may have attached to it a particular motto, and be accompanied with a sealed letter referring to such motto, and mentioning the competitor's address.

Communications, either on the above subjects, or on any other topic connected with horticulture, may be addressed to Mr. T. Dickson, Leith Walk, or to Mr. P. Neill, Old Fish Market Close, Edinburgh.

Geological Society.

Supposed native lead.

At the meeting on the 21st of February, an extract of a letter from Mr. J. R. Jones of Holywell to the President was read, giving an account of a specimen, presented by him to the society, of supposed native lead, found in a bed of granite in the neighbourhood of Holywell.

An

Submarine volcano.

An extract of a letter communicated by the hon. Henry Gray Bennet, describing a submarine volcano, which made its appearance on February the 1st, 1811, off the island of St. Michaels, in the Azores.

Cornish oxide of tin.

The reading of a paper by W. Phillips, Esq., entitled "a description of the Oxide of Tin, the production of "Cornwall; of the primitive crystal and its modifications "including an attempt to ascertain with precision the admeasurement of its angles mechanically, by means of the "reflecting goniometer of Dr. Wollaston; to which is "added a series of its crystalline forms and varieties;" was commenced.

The native oxide of tin appears to have been found in almost every district of Cornwall, and in the opinion of Mr. P. is by no means peculiar to the primitive rocks of that country. Particular crystalline modifications of this substance characterize particular veins.

Alluvial depositions of tin of considerable extent and depth have been found in several parts of Cornwall, which appears to be the only part of Europe, in which this metal occurs under these circumstances. The peculiar variety called wood tin has hitherto been met with only in these beds, or stream-works, as they are termed in the country; and these have also furnished the only specimens of gold hitherto found in Cornwall.

Among the specimens of tin in the collection of Mr. Phillips it may be observed occurring in granite, in mica slate, and in other varieties of schist, accompanied by chlorite, tourmaline, calcareous spar, schiefer spar, topaz, chalcedony, quartz, fluor spar and chlorophane, yellow copper ore, blende, arsenical pyrites, and wolfram.

Chemical lectures.

Mr. Singer will commence his course of Lectures on Chemistry, at the Scientific Institution, on Tuesday the 3d of March; they will be continued on each succeeding Tuesday, at eight o'clock in the evening.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

APRIL, 1812.

ARTICLE I.

On fresh-water Plants. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

I Ventured to observe in my last letter, that, without keeping to a certain classification in plants, when their dissection was pointed out, especially when no prints attended the work, it was almost impossible to comprehend it, though perfectly well instructed in the subject intended to be described. The natural system, which arranges certain figures together, and finds a similitude unmarked by the eye till the knife has dissected and pointed it out, is perfectly unconnected with any other selection. Thus, the grasses, water-grasses, ferns, mosses, fresh-water plants, and cryptogamia, are all so completely *unlike*, that to give their parts in a cursory manner, is to render them almost unintelligible. As long as the several divisions are arranged in the usual course of rind, bark, wood, &c., they may all be compared with the formation of trees: and so long as the wood is placed

The necessity of arranging dissections.

placed

placed in regular vessels, surrounded by the clear albumen, and containing the spiral vessels; and as these cylinders are sprinkled all over the interior, and the interstices filled with pith: they are known to be herbaceous, annual, or semiplants, and their formation is understood, and they will still bear a sort of comparison with trees and shrubs. But when the whole arrangement is overturned, and plants are found possessing parts unseen before, as in the fresh-water plants; or when in dissection the usual assortment of matter can no longer be recognized, and neither wood, bark, spiral vessels, &c. can any more be found, as is certainly the case in the marine plants; then it is absolutely necessary, to begin again the description, and by strict examination and trial discover anew the different uses, to which these parts can be applied.

Monsieur du Thouars' work.

It is this cursory method of giving every different sort of plant together, which makes the work of Monsieur du Thouars so difficult to understand: but it must be considered, that he delivered most of his book in lectures to his pupils, and probably exemplified them in the very best manner by living specimens, which would explain them to his hearers, though not to his readers.

Marine plants.

As I intend to show in this letter the formation of fresh-water plants, and in my next letter marine plants, it will not be amiss to draw first a sort of comparison between them, at once to prove the necessity of the arrangement just specified. Marine plants have no vessels. The whole is formed, whether thick or thin, by *blebs*, which allow of no communication one with the other: so that, if only a part of a fucus is drawn out of the water, this part dies, the rest not being capable of conveying to it any of its moisture. Each bleb has I believe a pore, but very difficult to find. They have no peculiar air vessels, though much air is mixed with the liquid within; which, not being confined in vessels, merely fills the blebs, and is retained by the cuticle of each surface. The only vessel to be found in a fucus, ulva, &c., is the line of life. They have no spiral wires, and of course no divisions of bark, wood, &c.; and appear in short to differ so much from every other plant, that their means of nourishment and existence must be wholly of another kind,

and

and require a very different process. But fresh-water plants are still capable of comparison with others, since, on dissecting them, you directly perceive the same sort of matter, as rind, bark, wood, &c. The line of life forms the centre, or meanders in it, and the air vessels are generally ranged next to it. I shall give the stem of the water lily as the first specimen (see fig. 1, Pl. VII). I have said, that the line of life is in the centre, where it forms a very thick circle. The whole is then divided into six parts by the bastard vessels, which pass from the centre to the circumference. Six large cylinders fill up a part adjoining the line. These are the air vessels, (see *a a*); and the rest of the space is occupied by the pith, sprinkled with wood vessels. The circumference is bounded with a few rows of bark, enclosing some inner bark vessels, and a rind surrounds the whole. This, with the figure, will give a general idea of it.

Fresh-water plants.

Stem of the water lily.

But it is necessary to give some description of the air vessel, which is really curious in its formation. It is a large cylinder, divided at every half inch of its length with a thin texture of pith: but lest this should not be sufficient to prevent insects from entering it, and choking up the vessel, as soon as the plant sinks in the water, a quantity of hairs, which are placed in circles in the interior, rise, and, meeting in the centre, not only aid to keep out the water, but run through every insect, that ventures to approach. I have often caught insects threaded on the hairs, but they are soon washed off. Sometimes the hairs remain in a horizontal position; but in general they rise and fall with the water. This, exciting my curiosity, made me anxious to apply to the solar microscope for the discovery of the mechanism, which regulated the motion of the hairs: and I found, that, though there was no spiral wire in the other parts of the plant, it was to be found in the hairs. The formation was simple, and merely caused by the contracting and dilating of the wire, as the hairs were drawn up, and ranged against the side of the cylinder, or placed themselves horizontally, their points meeting in the centre. The wood vessels are to be distinguished from the buds by their inferior size and and circular shape. The buds (as in most annual plants) proceed from the root; and of course have a stem shooting thence,

Its air vessels.

Hairs.

Wood vessels.

Buds.

thence, and never showing any leaves. The flower bud must therefore be growing all the time the stem is shooting, and does not appear till that stops, it is the same with the leaf, which has also a peculiar stem from the root, and but one leaf to each.

Not useful to classification.

It is impossible, that the formation of water plants in general can be of any use to classification, or to the selection of a natural method, as I once hoped it would be. Complete water plants indeed are outwardly known by those well acquainted with their general appearance, and with the classes. But the half water plants are culled from every genus, even those springing in the dryest land, yet varying, it should seem, from their species by growing in the water. What a light does this throw on the uses of the different parts of a plant! It is the water, which appears to operate on the interior formation of the stems, peduncles, and vessels of the leaf. Nor does this alteration seem to effect the conformation of the fruit or flower, which are the same as in the rest of the species. Not even the seed shows to the eye any difference, but into this latter fact I mean to make a farther inquiry. In the veronica beccabunga, where two or three are taken from many of the species growing in tolerably dry soils, the whole formation of the stem is altered; see fig. 9. Instead of a great portion of bark, eight or ten large air vessels supply its place. The rind is completely formed of cylinders of air and water divided. Instead of a wide row of wood, it has one line and eight circular vessels of the same, half wood, half clear albumen. It has indeed its spiral lines within these, and its line of life meandering in the pith: but this is all in which the stem resembles the usual veronicas, which are in the interior all alike, the water variety excepted: and what is curious, the anagallis and scutella growing only in boggy ground, not so wet as the usual situations of the beccabunga, has fewer air vessels, and more wood. The sisymbrium nasturtium, menyanthes trifoliata, ranunculus aquatica, potamogeton pusillum, and many others equally deviate from their species, and deserve the name of half-water plants, being more or less varied. That these should possess the spiral is not to be wondered at, since the leaves, being raised much above the water, require

Half water plants.

require it to turn themselves, and expose their upper surfaces to light and heat; which when they lie on the water (as in absolute water plants) is not necessary. The leaves also of the plants just mentioned, the veronica and menyanthes, have in their peduncles many air, and but one set of wood vessels; but the most curious part is a sort of perforation in the bottom of the peduncle, which is formed between the air vessels and lower cuticle to contain air, and support the leaf stem upright above the water.

May we not therefore conclude, that the air vessels are only intended to support the stem upright in a different element, and to raise it above, or depress it below the water, as the situation of the plant required? Could I but procure a plant, that had by change of climate become a sort of water plant (which I should suppose is possible) it would then be easy to see, whether the air vessels would form themselves to accommodate their structure to the element in which they reside. I cannot but be persuaded I have seen changes as great: will not a tree by degrees change its time of shooting? will not a plant often seek a more agreeable soil if near it? But time may enable me to show this in a more conspicuous manner. I am so fearful of advancing a single step beyond what specimens will absolutely disclose, that I would far rather leave a fact unaccounted for, than pass beyond what dissection will really justify or bring proofs of. Air vessels forming to accommodate to their element.

As it will not be possible, on account of the numerous figures, to give all I have to say on fresh-water plants in this letter, I shall conclude by showing the difference between a real water grass and a half-water one, or that which at one time of the year lies on water. The former is entirely composed of rows of air vessels, which between every three or four have a row of bark studded with wood vessels, very small, and as usual half wood, half albumen, but no spiral vessels. When the flower shoots, it is in thick threads from the root; slipping up between the leaf and its outward cuticle, as in all grasses. This thread is the line of life, with some wood surrounding it, and is a fresh proof, if it was required, that the female proceeds from this line, and the male from the wood. But the half-water grass is very differently Difference between a real water grass,

and a half-water grass.

ferently contrived, the upper face exactly resembles common grass, but to support it on the water it has long cylinders, which are merely of a loosened skin of that kind, which permits not water to penetrate. These are the air vessels, and support the grass perfectly dry on the water, where it swims, and defies both rain and wind, (see Pl. VIII, fig. 1 and 2, where *ff* are the air vessels; fig. 4, Pl. VII, being the water grass). By this it may be seen how gradually the plants approach to the state of perfect water plants, and if any other proof is wanted, the potamogeton lucens, which has generally a double stem, would show it. This grows constantly in the water; it is small, and requires few wood vessels to bring it support. It is almost wholly composed of vessels of air, one wood vessel being between each row; but it has a long circular stem in the interior, bounded by the line of life, and with a deep border of wood, up which the buds pass, as in all the fern and potamogeton genera, though some have several instead of a single one. To this specimen I shall add a sort of scirpus growing always in water, having its air vessels next the rind, as in figs 3 and 6, Pl. VIII, and merely threads tying the wood vessels in the middle, one to the other. The air vessels resemble in form those of the water lily, and are very different from the air cylinders in half-water plants, which I should before have mentioned. These are divided into compartments, as in Pl. VIII, fig. 5, *c d e*, which represents the veronica beccabunga, being half air vessels, half wood, so that the air enters only alternately; and they have no hairs as in the water lily, the wood part being wholly filled up.

Dissection of a scirpus.

Dissection of air vessels of the veronica.

No perspiration in plants.

The perspiration of aquatic plants was supposed to be uncommonly copious. I have so often troubled the public with the proofs of there being no perspiration in plants, that I shall not here repeat them, but merely give an explanation of the cause of the quantity of air found around almost all fresh water plants. There is in the bark juice (that is, the blood of the plant) a glutinous matter, which when moved catches the air in bubbles, and will continually enclose it, and cover the whole leaf and plant with vesicles of air, by which means it is prevented from sinking, let the rain beat ever so hard against it, or the wind attack it. I have seen

seen the bubbles of air formed by a brisk blowing wind cover a plant, till it appeared as if rolled in diamonds: but when I have drawn off the upper cuticle of the leaf, and subjected it to the solar microscope, not a single aperture could be discovered, and the marks proved as usual to be only the indentations of the pabulum seen through the cuticle.

To this I shall add the parts that belong to each different sort of plant, which will elucidate the subject, and show the confusion which must arise from not properly discriminating them.

I am, Sir,

Your obliged servant,

AGNES IBBETSON.

Different Parts of each various Sort of Matter found in the different Stems of Plants, and arranged in the manner they pass from the exterior to the interior Parts.

In trees in general.—Rind, bark, inner bark vessels, wood, spiral vessels wound round the inner wood vessels, line of life, pith; bud proceeding from the nearest line of life, whether in stem or twig. Structure of the stems of plants.

Fir trees.—Leaves cover instead of rind and bark, and thicken by degrees, having the inner bark vessels within the leaves: a thick row of wood, resembling the common wood, placed as a screen to guard the new wood from the deleterious effect of the juices: hard wood, line of life, pith: bud as usual in trees.

Shrubs.—Rind, bark, inner bark vessels, wood, spiral vessels, line of life, pith: bud as usual in trees.

Herbaceous Plants.—Rind, bark, inner bark vessels, wood, line of life within the pith, wood generally in rows, in number according to the length of the season: buds shooting from the interior of the pith, of course forming the line of life.

Annual and semiplants.—Rind, bark, inner bark vessels, wood in circular vessels, half wood half clear albumen, scattered in the pith, with the spiral vessels within the wood, and having the line of life within the pith.

Fresh-

Fresh-water plants.—Rind, air vessels, wood in scattered vessels all over the pith, being partly wood, partly albumen, but having no spiral vessels within, but the line of life in the centre, in a thick line: buds proceeding from the root.

Half-water plants.—Rind, bark, air vessels disposed in it with inner bark vessels; wood, either in rows or scattered vessels with albumen, and spiral vessels within; line of life meandering in the pith.

Marine Plants.—Rind, the rest vesicles of a glutinous matter, with a pore to each, but no communication from one to the other. Though an appearance of stalk, yet formed exactly the same as the rest of the plant, and without any vessels or lines except the line of life difficult to find, but in the fructification most plainly appearing.

The other parts of the cryptogamia will be given in my next letter.

II.

On the Zigzag Motion of the electric Spark. In a Letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

Zigzag course of the electric spark.

ALLOW me, through the medium of your valuable Journal, to communicate a supposition on a point, that seems to have been withheld entirely from public discussion: I mean the zigzag appearance of the electric spark passing from one body to another, as from a positive to a negative, &c. Partial to the science, but limited in experiment, or you might have had enough to prove a belief in the idea now formed; the only account I have ever heard at lectures was, that its own rapidity of motion condensed the air to such a degree, that it had to move from a solid, as it were, to a less dense medium, which seems to me impossible. My supposition is, that the fluid passes in a more direct line, according to the best or worst conducting substances presented to it. Our atmosphere, being a compound of oxi-

Owing to the

gen

gen &c., presents at once to the spark flying off the machine at least four known gasses, all, I have not the smallest doubt, differing in their conducting powers, were they separately tried. This point being ascertained, the phenomenon is at once accounted for: the fluid flies to the next best conducting gas from a worse, as it would from different portions of matter. I could advance more on this, only fear occupying too much space at the expense of more valuable communication than this from

Compound nature of the atmosphere.

Your most obedient servant,

I. PHŒNIX.

Liverpool, the 16th of Jan. 1812.

III.

Abstract of a Paper on Fermentation: by Mr. Gay-Lussac*.

It is fully demonstrated by the experiments of Lavoisier, as well as by those of Messrs. Fabroni and Thenard, that to produce alcoholic fermentation requires the concurrence of a saccharine matter, and a peculiar ferment of an animal nature. The circumstances favourable to fermentation have been long noticed: and it appears to be at present admitted, that it may be begun and continued without the assistance of any foreign matter, even of oxigen gas. It has been ascertained in fact, that, when the yeast of beer is introduced with sugar and water into a vessel, so as to fill it entirely, fermentation takes place in it in the same manner as in the open air: and hence it has been inferred, that the fermentation of the must of grapes, saccharine fruits, and grain, would take place, like that of sugar and yeast, without the contact of oxigen gas. But to render this inference legitimate, it must be presumed, that the ferment contained in fermentable substances is of the same nature as that of yeast. Mr. Thenard, to whom we are indebted for

Saccharine matter and a ferment requisite to vinous fermentation.

May be carried on without air,

in all cases as some have supposed,

which requires the identity of the ferment.

* Ann. de Chim. vol LXXVI, p. 245. Read to the Institute Dec. the 3d, 1810.

for an excellent paper on fermentation*, has accordingly adopted the opinion, that the ferment was in all cases identical. The experiments I have made have led me to a different opinion; and the principal object of this paper will be to show, that the fermentation of grape must cannot take place without the assistance of oxigen gas. Hence it follows, that the ferment of the grape is not of the same nature as yeast; or rather, that they are not both in the same state.

This opinion erroneous.

Preservation of animal and vegetable substances.

I was led to this inquiry by an examination of the processes employed by Mr. Appert for preserving vegetable and animal substances†. I had observed with surprise, that grape must, which had been kept unaltered a whole year, began to ferment in a few days after being poured into fresh vessels. It is in this way Mr. Appert prepares sparkling wines [*vins mousseux*] at all seasons of the year. This fact led me to suspect, that the air had some influence on fermentation, and suggested to me the following experiments.

The air influences fermentation.

Experiment with grape juice.

I took a bottle of grape must that had been kept a year, and was perfectly limpid; poured it into another bottle, which I corked tight; and exposed it to a temperature from 15° to 30° [59° to 86° F.]. In a week's time the must had lost its transparency; fermentation had taken place in it; and it was soon converted into a vinous liquor, sparkling like the best champagne. A second bottle, that had been kept a year, like the preceding, but was not exposed to the contact of air, gave no signs of fermentation, though placed in the most favourable circumstances for producing it.

Fermentation excited by the contact of air.

The same shown by another experiment.

I then took this bottle of grape must, cut it pretty deeply round the neck with a file, inverted it in a mercurial trough, and then broke off the neck, without suffering the must to come into contact with the air. One portion of the must I passed through the mercury into a jar containing a small quantity of oxigen gas, and another portion into a jar perfectly void of air. The first fermented in a few days:

* Ann. de Chim. vol. XLVI, p. 291. See Journ. vol. VII, p. 33.

† These processes, which are extremely simple, consist in putting the substances to be preserved into bottles, corking them very close, and then exposing them to the heat of boiling water for a longer or shorter time. See the instructions published by Mr. Appert.

the

the second gave no sign of fermentation in forty. On absorbing by potash the carbonic acid gas evolved during the fermentation of the first portion, a very little residuum was left; consequently the greater part of the oxigen gas I had added was absorbed.

Oxigen absorbed.

These results evidently prove, that must kept a long time cannot ferment without the contact of oxigen gas. But to obtain still greater certainty on this point, I analysed with Volta's eudiometer the air found in several bottles of must, that had been kept a year, and found in them no oxigen.

Farther confirmation of this.

I proceeded in the same way with the juice of gooseberries and grape must recently prepared, which had been exposed in well-corked bottles to the heat of boiling water, and obtained precisely the same results.

Si m with fresh juice.

It is very remarkable, that, when a fermentable juice, which has been kept a long time, is poured into another vessel, so that it would ferment from having been exposed to the contact of the air, it may readily be deprived of this property, by exposing it anew, in bottles closely corked, to the heat of boiling water. By this operation we perceive it loses its transparency, and afterward lets fall a slight sediment. During the fermentation of a very limpid juice a sediment is also deposited: but there is this difference between them; that of the latter is capable of exciting fermentation, but that of the former no longer enjoys this property.

Fermentation destroyed by heat and exclusion of air.

Sediment.

From these several results I have considered it as very probable, that grape must recently obtained would not ferment, if the grapes were pressed without the contact of air. Accordingly I took a jar, into which I introduced some small bunches of grapes perfectly whole; inverted it under mercury; and filled it five times following with hidrogen gas, in order to expel the smallest portions of atmospheric air. I then bruised the grapes in the jar by means of an iron rod, and exposed them to a temperature of 15° or 20° [59° or 68]. Twenty-five days after no fermentation appeared; though must, to which I had added a little oxigen, had begun to ferment the first day, and in a short time after fermented very briskly. In these last two experiments I observed, that the oxigen was almost wholly absorbed; but

Grape juice pressed out without the contact of air will not ferment;

but I cannot say whether it combined with carbon, or with hidrogen. I obtained a quantity of carbonic acid gas equal in bulk to a hundred and twenty times the oxigen gas I had added to the grape must; whence it is evident, that, if oxigen be necessary to the commencement of the fermentation, it is not to its continuance; and that the greater part of the carbonic acid produced is the result of the mutual action of the principles of the ferment and those of the saccharine matter.

unless the grapes were ripe, and then slowly,

In another experiment of the same kind as the preceding, a fermentation commenced at the expiration of twenty-one days, but the grapes were in a very advanced stage of ripeness: and besides, a portion of the same must, placed in contact with a little oxigen, had fermented in six and thirty hours after it had been prepared. Hence it is further evident from this experiment, that oxigen gas is singularly favourable to the developement of fermentation.

except oxigen be present.

Oxigen equally promotes the fermentation of animal substances.

This action of oxigen on fermentable juices is observable also in animal substances. I have seen bottles containing beef, mutton, fish even, and mushrooms, prepared at Mr. Appert's; and a month after these different substances were found to be perfectly good. On being exposed to the air, they soon putrefied, as fresh animal substances would have done. On the contrary, if they were replaced in bottles after having been in contact with the air a few hours only, and were then exposed to the heat of boiling water, they would keep a very long while. If however the bottles were badly corked; and particularly if the heat were not sufficiently prolonged; and all the oxigen contained in the bottles were not absorbed, putrefaction soon came on. In fact, by analysing the air in the bottles in which these substances have been well kept, we may convince ourselves, that it no longer contains any oxigen; and that the absence of this gas is consequently a necessary condition for the preservation of animal and vegetable substances.

Vegetable and animal substances preserved by occasional heating, even when air is present.

On reflecting, that putrefaction and fermentation never develope themselves instantaneously, I conceived, from the preceding results, that vegetable or animal substances might be preserved, without being deprived of the contact of air, by exposing them occasionally to the heat of boiling water

water. Accordingly I took some cow's milk, gooseberry juice, and a solution of gelatine, and exposed them to the boiling heat of water saturated with salt, at first daily, afterward every other day.

Two months after all these substances were perfectly good. The butter, that had collected on the surface of the milk, was very sweet, only it was a little harder than fresh butter; and the milk appeared a little thinner than before the experiment. I need not say, that some milk, gooseberry juice, and jelly, which I kept by way of comparison, soon altered.

Urine keeps long if air be excluded.

Urine, which is known soon to putrefy, and from acid, which it is at first, to become alkaline, will keep a long time in vessels closely stopped, when it has scarcely been in contact with the air: it retains its transparency, acidity, and smell; and no ammoniaco-magnesian phosphate is deposited, though sometimes uric acid separates. When urine is left in contact with a small portion of air, it absorbs its oxigen pretty readily, and then the decomposition stops: but if a sufficient quantity of air be present, a great deal of carbonate of ammonia is formed, and ammoniaco-magnesian phosphate is almost always deposited with the phosphate of lime. The decomposition of urine therefore, as we see, is not analogous to fermentation; since the latter, when once it has begun, goes on without the assistance of oxigen gas.

Essential difference in ferments,

Returning to fermentation, and considering, that sugar and the yeast of beer will ferment without the contact of air, while the must of grapes has not this property; we are forced to admit, that there is an essential difference between yeast of beer and the ferment of the grape. Yeast is solid, and nearly insoluble in water: ferment, on the contrary, in the state in which it is found in fermentable fruits, is liquid; or, if it be solid, it must be very soluble in their juices. It appears to me however, that it may be solid in a great number of substances, but in a peculiar state, and different from that of beer yeast. Still it is very possible, that there is but one ferment, and that its difference from the yeast of beer is to be ascribed only to a little oxigen. In this view it would be analogous to indigo, which is capable of oxidation and disoxidation.

but perhaps merely from oxigenation.

Fer-

Fermentation still mysterious.

Fermentation still appears to me however one of the most mysterious of chemical processes; particularly because it operates only gradually, and we cannot conceive why, when the ferment and the sugar are intimately mixed together, they do not act on one another with greater rapidity. We might be tempted to believe, that it is partly owing to a galvanic process, and that it has some analogy to the mutual precipitation of metals.

Theory of Mr. Appert's mode of preserving animal and vegetable substances.

Be this as it may, it seems to me, that we may clearly conceive how animal and vegetable substances are preserved by the process of Mr. Appert. These substances, by their contact with air, readily acquire a disposition to putrefy or ferment: but on exposing them to the heat of boiling water in vessels well closed, the oxigen absorbed produces a new combination, which is no longer capable of exciting fermentation or putrefaction, or which is rendered concrete by heat, in the same manner as albumen*. In fact it is observed, that a juice disposed to ferment, and perfectly clear, becomes turbid at the heat of boiling water, and then is no longer susceptible of fermentation, unless it be placed in contact with oxigen gas. In this case, if it be made to boil as soon as fermentation begins to take place, the fermentation is quickly stopped, and a deposition, of an animal nature, takes place. It may farther be observed, that beer yeast, which has been exposed to the heat of boiling water, likewise loses the property of exciting the fermentation of sugar. Now since must of grapes that has been boiled still retains ferment in solution, which, to produce fermentation, requires only the contact of air; we must conclude, that only the part which has absorbed oxigen, and which is probably in the same state as beer yeast, is capable of coagulating by heat.

Heat destroys the fermenting property of yeast.

This is the idea I have formed of the preservation of animal and vegetable substances: and if, as the experiments I have related seem to prove, oxigen be necessary to the developement of fermentation and putrefaction, it is evident, not only that the heat must be continued long enough

Requisites to

* Seguin has supposed albumen to be the true principle of fermentation. See Journ. vol. XV, pp. 332, 333. C.

to

the success of Mr. Appert's process.

to destroy or render concrete the matter, which has absorbed oxigen, and is calculated to excite fermentation; but also that the vessels, in which the substances are to be kept, must be stopped too closely for the air to penetrate them. It is very probable, from this theory, that all sorts of fruits may be kept a long time in hidrogen or nitrogen gas, provided they had absorbed no oxigen. We may conclude too, that, if grapes will keep a long time without fermenting, it is because the exterior coat does not admit the entrance of oxigen; not, as Mr. Fabroni has supposed, from an excellent analysis of grapes, because the ferment and saccharine matter are in separate cells. Lastly I consider it as possible, that, if an animal substance, milk for instance, could be obtained without the contact of air, it would keep a long time without alteration.

Fruits might be preserved in hidrogen or nitrogen gas.

Fermentation excited by galvanism.

From what has been said it might be expected, that fermentation might be excited in the must of grapes obtained without the contact of air, by immersing in it the two wires of a galvanic battery; and this in fact takes place. But an inference deducible from this is, that it is probably by increasing the electric energy of the various substances in contact, that atmospheric electricity so powerfully promotes the acescence of milk, broth, &c.

Action of electricity.

Matching of casks.

The experiments I have related throw some light on the brimstoning or matching of wines, which has been practised from time immemorial, without any one hitherto attempting to account for it*.

Sulphurous acid best for preventing fermentation.

Acids, particularly the mineral acids, may prevent fermentation by combining with the ferment, or altering its nature: but sulphurous acid acts like the other acids, and besides seizes the oxigen, which the wine may have absorbed,

* This process, which consists in burning in the casks, that are about to be filled with wine, a greater or les number of suslphuretted matches, or pieces of linen dipped in melted brimstone, might be managed much more simply and economically, by preparing concentrated sulphurous acid with a good apparatus, and afterward adding a small quantity of this acid to the wine intended to be brimstoned.

[In our cider counties, where a similar process is performed, it is usual, I believe, to sprinkle some aromatic seeds, as coriander, over the melted brimstone. C.]

or

or which remains in the casks. This proves, 1st, that fermentation cannot commence without the assistance of oxigen; and 2dly, that, at equal degrees of acidity, the sulphurous acid prevents fermentation better than any other.

Farther experiments intended.

My labours at present are far from complete. I have instituted many experiments, the results of which yet remain to be known, or which require to be revised; and I reserve them for a more extensive disquisition, that will embrace other objects. I confine myself therefore to this abstract, and shall conclude with observing, that very pure sugar, as well as manna, has the property of dissolving the yellow oxide of lead, and of acting afterward on colours in the same manner as the alkalis.

Action of sugar and manna on lead.

IV.

Note on Prussic Acid: by Mr. Gay-Lussac*.

Little information of late respecting prussic acid, though both decomposed and composed.

SINCE the discovery of prussic acid by Scheele, and the labours of Messrs. Berthollet and Clouet, nothing very important has been brought forward on the nature of this acid. Though the mobility of its elements has permitted it to be decomposed, and it has even been composed by passing ammoniacal gas over redhot charcoal, it has not yet been obtained perfectly pure, so that we know not under what form it would present itself in this state. I have endeavoured to solve this question, and I shall prove in this note, that prussic acid is not permanently elastic; that it forms a liquid much more volatile than sulphuric ether, since it boils at 26·5° [79·7° F.]; and that, owing to this property, at a temperature from 20° to 26° [68° to 78·8° F.] it dilates considerably the air or gasses with which it is mixed, communicates to them its properties, and then resembles a permanently elastic fluid.

Its form when pure.

Attempt to obtain it so.

Desirous of ascertaining, for the purpose of a particular inquiry, whether prussic acid might be obtained in the

* Ann. de Chim. vol. LXXVII, p. 128. Read to the Institute Feb. 1811.

gaseous

gaseous state, I decomposed prussiate of mercury by muriatic acid, as directed by Mr. Proust. After the air contained in the vessels had escaped, and a strong smell of prussic acid was perceivable, I received the gas over mercury. Thus I obtained several jars full of an elastic fluid, inflammable, and with a powerful smell, which appeared to me to be gaseous prussic acid. However, on continuing the process, I perceived, that drops of a peculiar liquid acquired the gaseous state, as soon as they reached the summit of the jar, and depressed the column of mercury considerably. The temperature then was at 20° [68° F.]; and the next morning, the temperature being only 12° [53·6° F.], I observed, that the bulk of the gas I had obtained was greatly diminished, and that a liquid was deposited in the jars, where there was none before. I had then no longer any doubt, that the prussic acid was a very volatile liquid; and after several trials, which I shall pass over, I succeeded in procuring it readily in the following manner.

Method of procuring it.

I took a tubulated retort, into which I put prussiate of mercury; and to the neck of the retort I adapted a curved tube, one end of which I inserted into a small two-necked phial, containing a mixture of chalk and muriate of lime; the chalk being intended to saturate the muriatic acid, that might escape from the retort, and the muriate of lime to retain the water. From this phial another tube proceeded to a second, containing also muriate of lime; and from this issued a third tube, terminating in a third phial with a ground stopple, intended to receive the prussic acid. The apparatus being thus arranged, and all the phials surrounded with a cooling mixture of two parts ice and one salt, I poured some slightly fuming muriatic acid into the retort, and applied to it a gentle heat. The prussiate of mercury soon dissolved, and the liquor appeared to boil. In fact vapours were evolved, which partly condensed in the neck of the retort, forming streaks like alcohol. The process was stopped the moment water began to rise; though more prussic acid might still be obtained: but it is better to separate the first product, and afterwards resume the process.

Rectification of the acid.

All the prussic acid commonly condenses in the first phial. If no water pass over, the muriate of lime remains

 solid,

solid, though immersed in the prussic acid. If, on the contrary, a certain quantity of water have passed over, two very distinct strata of liquid will be obtained; the lower, an aqueous solution of muriate of lime; the upper, prussic acid. This acid is commonly a little coloured in the first phial. To rectify it, as soon as you think proper to terminate the distillation, take out the tube communicating with the retort; stop the aperture by which it entered the phial; and, after removing the frigorific mixture, that surrounded the latter, heat it very gently, either by means of a water-bath or charcoal, so as not to raise the temperature above 30° or 35° [86° or 95° F.]. When this distillation is finished, take away the first phial; and, after the prussic acid has remained a few hours in contact with the muriate of lime in the second phial, pass it over into the third by means of a gentle heat. The rectification is then finished.

Properties of pure prussic acid.

Prussic acid thus obtained is a colourless liquid, as clear as water. Its taste, at first cool, soon becomes acrid and irritating. Though rectified several times over chalk, it faintly reddens litmus paper; but the blue colour returns, as the acid evaporates. Its density at 7° [44·6° F.] is 0·70583. Its volatility is very great, for it boils at 26·5° [79·7° F.]: at 10° [50° F.] it supports a column of mercury of 0·38 met. [14·95 inches]; and at 20° [68° F.] it quintuples the bulk of the air or gasses, with which it is mingled. This property renders the employ of the apparatus I have described indispensable; for, if it were poured from one vessel to another in the open air, a very large quantity would be lost. This property also explains why it has been said by some chemists, that prussic acid may be obtained in the state of a permanently elastic fluid.

Its great volatility.

Freezing point.

Prussic acid exposed to a frigorific mixture of two parts ice and one salt constantly congeals, and frequently assumes a regular figure. I have sometimes seen crystals of this acid resembling those of fibrous nitrate of ammonia. It remains solid at a temperature of —15° [5° F.], but above this point it liquefies.

Singular phenomenon.

The great volatility of this acid, and its congelation at —15° [5° F.], occasion it to exhibit a remarkable phenomenon. If a single drop be exposed to the air at the extremity of a

glass

glass tube, or, which is still better, on a piece of paper, it instantly freezes. This congelation, produced by the evaporation of the prussic acid itself, is, I believe, the only one of its kind; for, among all the very volatile liquids, there is not one that freezes at a temperature so little remote from that of melting ice.

I have studied the chemical properties of prussic acid, prepared as I have just mentioned, and shall make known the principal in another paper.

V.

Abstract of a Paper on Triple Salts: by Mr. Gay Lussac*.

The acid of trisules commonly divided equally between the bases.

THE object of this paper is to show: 1, that, in triple salts the acid is commonly divided between the bases in two equal proportions. This is the case in the triple tartrates and oxalates; in the ammoniaco-magnesian sulphate; in the triple sulphate of zinc and ammonia; &c.

Elements of triple compounds would form binary ones;

2. That in a triple compound the elements united two and two form possible binary compounds. For example, the nitrate of ammonia, which is composed of oxigen, nitrogen, and hidrogen, when decomposed by fire yields water, and gaseous oxide of nitrogen: while, on the other hand, this salt is the result of two binary compounds, nitric acid and ammonia.

as well as those of higher compounds.

3. That the vegetable and animal substances, which are composed of three or four different matters, also give rise to binary compounds, that are possible, or generally known.

The same elements in the same proportions may form different compounds.

4. That we may form an idea of the different nature of several substances containing the same elements and in the same proportions; if we admit, that the binary products of the elements combine in different ways with each other, or merely with one of the elements.

* Ann. de Chim. vol. LXXVII, p. 185. From a paper read to the Soc. of Arcueil, February, 1811.

5. That

5. That we may imagine so many more compounds containing the same elements in the same quantities, as we can conceive the binary combinations, formed by the elements of these compounds, to be more numerous.

Neutral compounds indicate the capacity of combustibles for oxigen.

6. That salts, or other compounds, being neutral, though formed of an acid containing an excess of oxigen, and a base that is still combustible; we may admit, that the base saturates the excess of oxigen of the acid; and that hence results a point of saturation, well adapted to determine the capacity of combustibles for oxigen. For instance, the neutral nitrate of ammonia, being decomposed by heat, yields as products water, which is neutral, and gaseous oxide of nitrogen, which must be neutral also.

Density of nitrous acid gas.

7. That nitrous gas and oxigen gas, in combining to produce nitrous acid gas, experience an apparent condensation of bulk, which is precisely half the total bulk of the two gasses, whence it follows, that the density of nitrous acid gas is 2·10639, that of atmospheric air being 1.

VI.

Analysis of large-leaved Tobacco, Nicotiana tabacum latifolia and angustifolia: by Mr. VAUQUELIN*.

Tobacco probly contains a peculiar principle.

THOUGH there can be no doubt, that the various methods employed for preparing tobacco modify, each in its own way, some of the principles contained in this plant, yet the changes experienced by these principles cannot entirely destroy their peculiar properties; otherwise, it is evident, that tobacco might be made from a great number of herbaceous plants, which is not the case. Reason therefore leads us to conclude, that there exists in the nicotiana at least one substance, not to be found in the other plants, from which attempts have been made to manufacture tobacco in vain.

Analysis undertaken.

These considerations have led us to undertake a careful chemical analysis of the different species of nicotiana em-

* Ann. de Chim., vol. LXXI, p. 139.

ployed

ployed in manufacturing tobacco, as well as of the tobacco of different manufactories, both French and foreign.

We were of opinion, on entering upon this inquiry, that some advantages might result from it to the manufacturer with respect to the preparation of tobacco; or that at least the theory of chemistry might derive from it some principle, by means of which it could give a satisfactory explanation of the changes, that might take place in the matters entering into the composition of tobacco. Advantages to be expected from it.

I ought here to mention, that I have been assisted in this long and laborious research by Mr. Robiquet, a very well informed young apothecary of Paris, and Mr. Warden, American consul, who devotes the leisure moments afforded him by his office to the practice of chemistry. Persons engaged in it.

After having bruised the leaves of nicotiana latifolia in a marble mortar, we wrapped them in a linen cloth, and subjected them to the action of the press. To separate all the soluble matter they might contain, this operation was repeated three times, with the addition of a little water. Process.

Though the cloth was of a pretty close texture, the juice retained a large quantity of green matter in suspension, which was separated by filtration through blotting paper. The green matter that remained on the filter, was washed and set apart, and will be noticed hereafter.

Examination of the filtered Juice.

1. This juice strongly reddened litmus paper, a proof that it contained a free acid. The filtered juice examined.

2. Oxalate of ammonia, by the copious precipitate it formed, demonstrated the presence of lime, and consequently of some calcareous salt.

3. Nitrate of silver threw down a copious precipitate, which was not wholly dissolved by nitric acid; whence we may infer, that it was partly formed by a muriate.

4. The infusion of galls, and the mineral acids, indicated by the tolerably bulky brown precipitates they occasioned, the existence of some animal matter, particularly of albumen.

5. Heat raised to 80° of R. [212° F.] confirmed this by occasioning a copious coagulation.

6. Acetate

6. Acetate of lead formed a very copious grayish precipitate, which dissolved in great part in distilled vinegar.

Examination for malic acid.

The effect of acetate of lead on this juice inducing us to suspect the presence of malic acid, we precipitated by means of acetate of lead a pretty large quantity of the liquor coagulated by heat; and afterward passed through this precipitate washed and diffused in water a stream of sulphuretted hidrogen gas, till there was a slight excess of it.

The intention of this was to reduce the lead to a sulphuret, and thus separate it from the matter with which it had been united. To facilitate the precipitation of the sulphuretted lead, we heated the liquor, and filtered.

The liquor thus filtered was cautiously evaporated to the consistence of a sirup. In this state it had a very acid taste, strongly reddened infusion of litmus, and formed with alcohol and ammonia copious sediments, which, while they indicated the presence of animal matter, proved, that a portion of it had been carried down with the lead in its precipitation.

Animal matter.

The acid dissolved in alcohol.

Hoping that the acid contained in this thickened liquor would prove soluble in spirit of wine, and that we might thus separate it from the matter it held in solution, we treated it hot with this menstruum at 40° [sp. gr. 0·817]. In fact, as soon as the mixture of these two liquors took place, a copious coagulation was produced, and the alcohol became coloured, first yellowish, then brown red, and was found to be acid.

The matter not dissolved by the spirit of wine was whitish, partly dissolved in water, and its solution was precipitated by the acetate of lead like the acid itself.

Oxalate of ammonia occasioned in it a precipitate, and this substance, placed on burning charcoal, left a residuum of carbonate of lime. Lastly, we found, that this substance was formed in great part of malate of lime, which retained some portions of vegeto-animal matter.

The animal matter strongly attracted by the acid.

The greater part of this vegeto-animal matter, which we had endeavoured to separate by means of alcohol, having dissolved in it by the help of the acid, we saturated the latter with ammonia, which threw down a considerable flocculent

flocculent sediment, the properties of which were perfectly similar to those of animal matters. Notwithstanding this saturation, nutgalls still produced a very evident precipitate in the liquor; whence we perceive there exists a very great affinity between the acid and this animalized principle.

This acid, purified as much as possible, exhibited all the characters of malic acid; that is, it imparted considerable consistency to water by evaporation, did not crystallize, gave with acetate of lead a precipitate soluble in distilled vinegar, swelled up in the fire giving out a smell of burned sugar, and was converted into oxalic acid by the nitric acid. The acid was the malic.

Thus the acetate of lead had thrown down at once malic acid, a great deal of coloured vegeto-animal matter, and a little malate of lime. The last appeared to have been carried down in combination with the malate of lead, and to have been redissolved by the malic acid, as fast as it was separated by the sulphuretted hidrogen. Malate of lime separated,

In several experiments, in which we thus precipitated tobacco juice by an excess of acetate of lead, we in like manner found again malate of lime in the malic acid.

A large quantity of malate of lime therefore exists in tobacco, which may be obtained directly by evaporating the juice of the plant to two thirds. which exists in the plant largely.

As soon as we had completely ascertained the nature of the acid, we returned to the juice of tobacco into which we had poured an excess of acetate of lead, to treat it also with sulphuretted hidrogen. We had obtained a very transparent liquid, of a lemon colour, which retained the exact smell and all the acrimony of the unaltered juice. Suspecting, that this taste depended on a volatile oil, we distilled the liquor, and obtained a product, that had a slight herbaceous smell, and but little taste. The juice deprived of acid distilled.

The concentrated portion, that remained in the retort, emitted, on the addition of a little potash or ammonia, a strong smell, which was so pungent, that, if snuffed up with a little force, it occasioned sneezing and tears. We repeated the experiment by adding potash to a more considerable quantity of matter, and distilled, after having diluted it with a lit- Matter left in the retort. Distilled with the addition of potash.

a little water. The new product, which we obtained in this operation, had the smell of tobacco smoke, was extremely acrid, and produced a similar sensation to that occasioned by a pinch of snuff taken with such force as to get into the throat.

New product. As this product was alkaline, we suspected, that this principle, whatever it might be, was rendered volatile only by means of ammonia, arising from the decomposition of an ammoniacal salt in the tobacco; since, when the liquor contained an excess of acid, we did not obtain the same result. However, in a similar process, conducted it is true with dry tobacco, we obtained a product, the smell and taste of which were at least equally striking, though the liquor that yielded it contained a free acid. For the rest, we were never able perfectly to isolate this acrid substance, and even the greater part remained in the retort. Hence it appears, that the malic acid diminishes the volatility of this acrid principle.

Attempt to obtain it separate. In order to obtain this principle separately, we evaporated, by a very gentle heat, the liquor it contained, and treated it with alcohol at 40° [0·817], which in fact separated it from the other matters. On afterward evaporating the alcohol, we remarked at the surface of the liquid some traces of a brown oil; and the portion that distilled over became more and more acrid, as the process approached its conclusion. This nearly solid oil, when thrown on burning coals, emitted a thick smoke, and such a strong smell of tobacco, that it was insupportable.

Nitrate of potash. The alcoholic solution yielded on cooling some nitrate of potash.

The acrid principle. The acrid principle in question has little smell when dissolved in water; which shows, that it is not very volatile. It appears very difficult to destroy; for, if it be mixed with a pretty large quantity of oximuriatic acid, it still retains all its acrimony, after the acid has evaporated spontaneously.

Probably peculiar to tobacco. The peculiar degree of volatility and acrid taste of this substance seem to indicate, that it is a principle belonging exclusively to the genus nicotiana; and which is consequently new, since those chemists, who have analysed this plant, have not spoken of it, at least as far as we know.

Not produced artificially. Hence we may conclude, that this principle, which is found

found also in prepared tobacco, as we shall show in another paper, is not altered by the different operations it undergoes; and consequently is not produced by any change in the constitution of the materials of the plant.

The following are the substances we have thus far found in the juice of tobacco: Substances found in the juice.

1, An animal matter:
2, Malate of lime with excess of acid:
3, Nitrate of potash:
4, Muriate of potash:
5, A peculiar acrid principle.

Now we know these different substances contained in the juice of tobacco coagulated and filtered, we shall point out the results of the successive experiments made on the green fecula, on the coagulum obtained during the boiling of the juice, and on the woody residue.

Examination of the green fecula; The green matter btained by filtering tobacco juice, being treated with alcohol, left as an insoluble residuum a grayish substance, tolerably compact, yielding on distillation a great deal of carbonate of ammonia, partly crystallized and partly dissolved in water, a thick fetid oil, and a coal of difficult incineration, which yielded a little lime, proceeding no doubt from a portion of decomposed malate. It appears, that this matter is a portion of insoluble vegetable albumen. As to the colouring portion of the fecula dissolved by alcohol, it differed in no respect from the green fecula of vegetables.

of the matter coagulated by boiling; We have said, that a pretty considerable coagulum was formed on boiling the filtered juice. This coagulum is an albuminous vegeto-animal substance; for it emits the same smell as horn when burning, and yields a great deal of ammonia; but it is remarkable, that it leaves a great deal of lime after it is burned.

To find whence this earth could be derived, we treated a portion of this animal matter with muriatic acid very much diluted. The solution, filtered and saturated with ammonia, yielded by cautious evaporation a granular powder, which was also malate of lime. What had not dissolved in the muriatic acid was pure vegeto-animal matter.

and of the woody residue. Something remains to be said respecting the woody residuum.

duum. After it had been well washed, we treated it with alcohol, to take up all the green resin it contained; and we afterward subjected it to the action of weak nitric acid, which extracted from it various calcareous salts. At first, on saturating this acid by ammonia, we obtained a flocculent matter, which, when decomposed by sulphuric acid, yielded an acid liquor, that precipitated lime water in large flocks, which oxalic acid does not. However, as we suspected the existence of oxalate of lime in this woody residuum, we cautiously evaporated a portion of the same liquor, and obtained a few crystals of oxalic acid.

Method of separating phosphate from oxalate of lime.

Phosphate of lime may be pretty accurately separated from oxalate of the same base, when they are dissolved in nitric acid, by adding ammonia to the solution, so that a slight excess of acid shall remain. The oxalate of lime will be precipitated for the most part in a pulverulent form, while the phosphate of lime remains in solution; and this may be precipitated afterward by a larger quantity of ammonia.

The mother water of these crystals, evaporated to dryness and calcined, yielded us phosphoric acid. We also perceived in the nitric solution traces of calcareous salt, which we separated by evaporation of the liquor, and which we found to be malate of lime.

Matters in the woody residue.

Thus the nitric acid took up malate, phosphate, and oxalate of lime, from the woody residuum.

Lastly the woody matter, after having been treated by these different agents, left, when burned, some ashes, composed chiefly of silex, with a little lime and iron.

Another process for obtaining the acid principle contained in tobacco leaves.

Another mode of obtaining the acid principle.

Instead of precipitating by means of acetate of lead the juice of tobacco coagulated by heat, as we have related above, the juice may be evaporated by a gentle heat, and, when reduced to about one fourth, suffered to cool. It will then deposite a pretty large quantity of malate of lime in granular crystals, which will become opake by exposure to the air. On boiling down the solution still farther, it will yield fresh quantities of the same salt; and lastly, when

it

it has acquired such a consistence, as will not allow the saline particles to unite, it is to be treated with alcohol, to dissolve the free malic and acetic acids, the acrid matter, and the sal ammoniac; and to separate the animal matter, which the heat could not coagulate on account of the acids, that held it in solution.

The alcohol, containing in solutions the matter just mentioned, is to be evaporated in a retort. It will carry over nothing with it. The liquor remaining in the retort is to be concentrated anew, and treated a second time with highly dephlegmated alcohol, to precipitate some portion of animal matter, that was dissolved in the former operation by means of a little water.

This second portion of alcohol being evaporated in its turn, what remains is to be dissolved in water; the malic and acetic acids are to be saturated accurately with potash; and the whole is to be distilled to dryness, taking care that it does not burn. The water obtained, though clear and colourless, is insupportably acrid; and what is left in the retort still retains the same property: but on redissolving it several times in water, and distilling it, the operator will at length deprive it almost wholly of its acrid taste, and obtain the principle that produces it dissolved in distilled water.

We have not yet satisfactorily ascertained the nature of the matter that accompanies it, and which dissolves with it in the alcohol. This matter has a yellowish red colour; and swells up and is converted into a coal in the fire. Matter that accompanies it.

If, after this matter has been divested as far as possible of the acrid principle, the residuum be urged with a stronger heat, an oil will be obtained, and muriate of ammonia will sublime. It likewise elds ammonia from the decomposition of the muriate by the potash of the malate and acetate, which the heat decomposes.

From the experiments here related it follows, that the juice of nicotiana latifolia contains

1, A large quantity of animal matter of an albuminous nature: Contents of tobacco juice.

2, Malate of lime with excess of acid:

3, Acetic acid:

4, Nitrate and muriate of potash in notable quantity:

5, A

5, A red matter soluble both in alcohol and in water, which swells up considerably in the fire, and of which I do not well know the nature:

6, Muriate of ammonia:

7, Lastly, an acrid, volatile, colourless principle, soluble both in water and in alcohol, and which appears to be different from all that are known in the vegetable kingdom. It is this principle, that imparts to prepared tobacco the peculiar character, that renders it easily distinguishable from every other vegetable preparation: this will be proved in a subsequent paper, which we shall give on snuff.

This principle possibly an oil.

It is possible, however, that this principle may be nothing but a very thin oil, which, on this very account, would possess a certain degree of volatility, and the property of dissolving in water and vegetable acids, as common volatile oils do; for on treating dry prepared leaf tobacco directly with alcohol, we obtained, independantly of the acrid principle, a brown oil, that had nearly a similar taste.

It may be conceived, that this matter existed originally in the plant in the state of volatile oil; and that it has been thickened, and in some measure resinified, by the progress of vegetation and desiccation.

It might be supposed too, with equal probability, that the thick oil, of which we have just spoken, is a part of the green resin, that owes its acrid taste to a portion of the volatile principle, which has combined with it. At least there is no doubt, that prepared tobacco owes the greater part of its distinguishing properties to the acrid principle and the oil that exist in the leaf of the nicotiana, for these two substances produce the same sensations in the mouth and in the nose as tobacco itself.

Smoking tobacco,

In smoking tobacco these sensations are modified by the empyreumatic oil, pyroligneous acid, and ammonia, that are formed during combustion; yet we still distinguish very sensibly those arising from the substances in question.

rendered milder by water.

By passing the tobacco smoke through water, as is done in certain countries, the smell and taste of these two peculiar substances are rendered more mild and agreeable.

In a subsequent paper we shall give an analysis of the dried leaf tobacco, and of snuff, prepared in different countries,

countries, in order to make known the effects of art on this plant*.

VII.

Mineralogical and Chemical Examination of Magnesite, the native Magnesia of Werner: by Messrs. HABERLE *and* BUCHOLZ†.

THE specimens employed in this description and analysis were sent by counsellor André of Brunn from Hrubschitz, in the lordship of Groman, in Moravia. Those analysed by Mr. Mitchel, which led Werner to make a separate species of this mineral in his system, were from the same place. Mineralogical description.

Various inaccurate oryctognostic descriptions, which have been given in the different elementary works of France and Germany, lead us to wish for a fresh examination of this substance. Thus Reuss and Suckow have said, that magnesite shines when rubbed, that it is light, and that its specific gravity, according to Gerhard, is 0·31: but certainly Gerhard was mistaken, or spoke of some other substance. Guyton too, when he gives its specific gravity at 2·162, is wrong; as the late experiments of Haüy on the same substance show. They who say, that this substance is difficult to break, and that it adheres slightly or not at all to the tongue, have fallen into a still greater errour. Errours in various writers respecting it.

Brongniart, in his Elementary Treatise on Minerals, describes this substance too slightly, or repeats the errours of others; as for instance when he says, that it is greasy to the touch. In fact he brings under the species magnesite too many minerals, which differ both in their oryctognostic and chemical principles. Thus he unites the meerschaum, or plastic Brongniart.

* For an account of the empyreumatic oil of tobacco, and its poisonous effects on the animal economy, by Mr. Brodie, see Journal, vol. XXX, p. 305.

† Ann. de Chim. vol. LXXIV, p. 65. Translated from Gehlen's Journal, N° 31 and 32, by Mr. Tassaert.

plastic magnesite, with the siliciferous carbonated magnesia of Haüy. But the meerschaum is nothing but a hydrate of silex, which may be made smooth by friction; and this Brongniart gives as one of the characters of magnesite. He is mistaken too when he makes the magnesite of Baudissero and Castellamonte, near Turin, a subvariety of plastic magnesite; for this substance belongs to the magnesite of Mitchel. As to his magnesite of Vallecas, it is a true meerschaum, which we cannot join with Mitchel's magnesite; from which it differs greatly in specific gravity, being of 1·6, and also in tenacity. The plastic magnesite of Salinello of the same author is obviously a variety of steatite, as its analysis shows.

Haüy. Haüy appears to be utterly unacquainted with meerschaum, for, in vol. IV, p. 443, of his work, he confounds it with the red Turkish clay, of which pipes are sometimes made; but this is nothing but a bolar earth.

Analyses. It is pleasing to see how nearly the analyses of magnesite made by Mitchel, Lampadius, Klaproth, and Bucholz, agree. This shows how greatly Wondraschek was deceived, when he ascribes to it 20 per cent of water. We perceive too, that Giobert was mistaken in his analysis of the stone of Baudissero, or that it is not a magnesite. We must also question the accuracy of the analysis of the magnesite of Castellamonte, published by Guyton; which consists, according to him of 46 carbonic acid, 12 water, 26 magnesia, and 14 silex, yet is insoluble in water.

Magnesite described. Magnesite is found in rounded pieces, sometimes as large as a man's head, and of an earthy aspect.

It is always dense, and formed of earthy particles, dull and of a meagre feel; sometimes with fissures in its interior, but never with rounded cavities; and sometimes, but very rarely, a siliceous nucleus resembling chalcedony is found in the centre.

Specific gravity. The specific gravity of magnesite, when it has been suffered to imbibe as much water as it will take up, is 2·881; otherwise only 2·456. Haüy has found, that the magnesite of Castellamonte, which contains 14 per cent of silex, was of the specific gravity of 2·781, when thoroughly soaked in water, and of 2·175 previously.

The

The hardness of this stone is less than that of fluate of lime, which scratches it; and greater than that of calcareous carbonate, which it scratches: but it effloresces at the surface, or perhaps it is a combination and absorption of water that takes place, and then it becomes friable and very tender, so as to colour the fingers, and readily yield an earthy powder. In all cases it does not become smooth when rubbed, or change its colour. Hardness.

Its cohesion is trifling; and it is the more easily friable in proportion as it contains less of alumine and silex.

Its fracture is conchoidal, inclining to even, dull, and a little rough to the touch, but never smooth. The fragments have no determinate form, their angles are more or less acute. Fracture.

It has no transparency, but the thinnest edges are sometimes translucid. Opake.

Its colour is always of a yellowish gray, or a yellowish dirty white, and black spots or figures are seen proceeding from the surface to the inside. Sometimes too it appears marbled with yellowish gray and blueish gray spots, particularly the siliceous varieties. Colour.

When magnesite is rubbed on woollen cloth it acquires the vitreous electricity. Electricity.

It is not phosphorescent.

It is strongly adhesive to the tongue; and when put into water it absorbs 9 or 10 per cent, and becomes transparent at the edges, but water does not dissolve it. If triturated with water, it does not form an adhesive paste, but a mass easily reducible to powder by drying; and it emits a smell of magnesia, not of alumine: but the impure pieces have a strong argillaceous smell, when thus treated. Adhesive to the tongue. Action of water. Smell.

It effervesces with concentrated acids, and is dissolved by them, in 24 or 36 hours; only when it contains silex, this is not dissolved. Effervescent.

It is infusible before the blowpipe, or with the strongest fire; but it loses its carbonic acid, contracts in its dimensions, and grows so hard as to scratch glass. Infusible.

According to Mr. André, magnesite is found accompanied with common and earthy talc, as well as with meerschaum, and even magnesian limestone [*bitter kalk*], in a stratum of serpentine in a state of decomposition. Where found.

In

In the superior strata of the decomposed serpentine green chalcedony and opal are found.

The magnesite of Castellamonte, near Turin, is also found in strata of serpentine and talc.

Its uses.

This stone might be used for manufacturing sulphate of magnesia. According to Giobert it is used in Piedmont in the porcelain manufactory. We are informed also, that the meerschaum of Vallecas in Spain is employed in the porcelain manufactory at Madrid.

Stones with which it may be confounded.

Care must be taken, not to confound magnesite with talc or lithomarga; and though Werner says, that there are some varieties of magnesite which are soft to the touch, I believe they are only fragments approaching to steatite, and then they become smooth when rubbed.

Its carbonic acid not absorbed from the air.

We must likewise reject the opinion of Giobert, who thinks, that the magnesite of Baudissero contains no carbonic acid while it remains in the earth, but attracts it subsequently from the air; for hitherto no magnesia combined with water alone has been discovered. If magnesia be found pure, it is always intimately mixed with a large proportion of silex, as is the case in steatite, or speckstein. The analysis published by Giobert too must be considered as faulty. According to him magnesite is composed of

Giobert's analysis.

Magnesia	68
Silex	15·6
Carbonic acid	12
Water	3
and casually it contains sulphate of lime	1·6
	100·2

Analysis.

Chemical analysis.

1st variety described.

A. This first variety has the greatest specific gravity, and the slightest degree of cohesion; emits no argillaceous, but a slight earthy smell; is strongly adhesive to the tongue; has a yellowish white colour, and little figures are observable in it. Externally it is friable, and sometimes soils the hands on touching it.

Action of acids on it.

a. When small bits of this stone are thrown into sulphuric, nitric, or muriatic acid, they dissolve but slowly at the common

common temperature, and some flocculent matter of a light reddish colour remains. By the assistance of heat the solution proceeds a little more speedily, and is complete. If the bits of stone were of a tolerable size when thrown into the acid, the effervescence took place only at their edges, or in a crack where two edges joined. The most remarkable circumstance was, that the magnesite fell to powder, before it dissolved, which facilitated its solution.

b. In order to ascertain the quantity of carbonic acid contained in this stone, we took 100 grs of magnesite in fine powder, and threw them into thrice their weight of fuming muriatic acid diluted with an equal quantity of water. The mixture was made in a very tall vessel, previously weighed; which could be heated by placing it on a plate of iron not sufficiently hot to raise any vapours. When the effervescence had ceased, the loss was found to be 52 grs. A few slight flocks, floating in the liquor, disappeared on raising the temperature. To avoid all errour, this experiment was repeated without heat, in a very tall vessel stopped with a perforated cork. In eight hours the effervescence had ceased, and the solution was complete, except a few white flocks, which disappeared during the night. The loss was precisely 52 grs.

Treated with muriatic acid, with and without heat.

c. On exposing 100 grs of magnesite to a red heat for an hour in an open crucible, they also lost 52 grs. The residuum, which was of a reddish white, was dissolved in sulphuric acid diluted with water, without the least effervescence: a slight flocculent precipitate of oxide of manganese remaining. This stone therefore contains no water; and hence no doubt arises the tolerable degree of hardness it possesses, as well as the slowness with which it dissolves in acids.

Exposed to a red heat.

d. The solution of experiment *c* was evaporated to dryness, and water poured on the residuum to the depth of two inches. The whole of the salt was redissolved, except a few slight flocks of oxide of manganese, mixed with a little sulphate of lime. On evaporating, and crystallizing at several periods, a little more sulphate of lime was deposited: but the whole, including the oxide of manganese, did not amount to a quarter of a grain. No silex was found in this stone.

Sulphuric solution evaporated.

e. To

Muriatic solution precipitated by ammonia.

e. To the second solution of experiment *b* a little more muriatic acid was added, and it was then supersaturated with ammonia. This threw down a reddish precipitate, which, when well dried, weighed a grain; but by exposure to a red heat it was reduced to half a grain. By a separate experiment this was found to be alumine, containing oxide of iron and manganese. These oxides we separated by dissolving in nitric acid the reddish residuum left by the stone after being heated redhot. The weight amounted to a quarter of a grain. On afterward supersaturating the nitric solution with ammonia, a few flocks of alumine were obtained, which were colourless.

Its component parts.

From the preceding analysis it appears, that this stone is an anhydrous carbonate of magnesia, containing a few atoms of lime, alumine, oxide of iron, and oxide of manganese, which appear to give it its colour. The proportions of this stone, and the pretty considerable hardness it possesses, are surprising. It contains in 100 parts

Magnesia	48
Carbonic acid	52
	100.

Artificial carbonate of magnesia always contains water, though its proportions differ,

From the properties of this stone it appears, that nature possesses peculiar means of producing anhydrous carbonate of magnesia: for, from the experiments I have made and published in Trommsdorff's Journal, the principles of carbonate of magnesia may vary, according to the mode in which it is prepared, but in all cases it contains a large quantity of water. If a solution of sulphate of magnesia be precipitated cold by subcarbonate of soda, we always obtain a carbonate formed of

when precipitated cold,

Magnesia	33
Carbonic acid	32
Water	35
	100.

In this process a large quantity of subcarbonate of soda must be employed, because part of it passes to the state of carbonate; but then the carbonate of magnesia obtained is the

the lightest possible, and very bulky; unless this be prevented by some mechanical operation, as strong pressure, which however does not alter its component parts.

If, instead of operating with the solutions cold, they be mixed at a boiling heat, 100 parts of the carbonate of magnesia produced will contain

Magnesia	42	precipitated hot, or
Carbonic acid	35	
Water	23	
	100.	

The carbonate of magnesia prepared by passing a stream of carbonic acid gas through water, holding carbonate of magnesia in suspension, or by filtering and leaving to spontaneous evaporation the liquor obtained after precipitating one part of sulphate of magnesia by four parts of subcarbonate of soda, contains in 100 parts

Magnesia	30	saturated by exposure to carbonic acid.
Carbonic acid	30	
Water	40	
	100.	

Here it appears, that the first and third processes approach near each other in the proportions of magnesia and carbonic acid; and, if we leave the water out of the question, they differ but little from the natural stone, though this contains a still larger proportion of carbonic acid.

B. The second variety of magnesite greatly resembles the first in the colour and marbling; but it is harder, and not so heavy. It is also less adhesive to the tongue, and emits a perceptible smell of alumine. 2d variety.

a. 100 grains of this stone in whole pieces, treated as in experiment *A b*, left 51 grs. At first a brisk effervescence took place, after which the stone fell to powder. In twelve hours the whole was dissolved, except a few light flocks, which disappeared by agitation. Treated with muriatic acid.

b. By an hour's calcination this stone lost 52 grs. The pieces had still a slight cohesion, but might easily be rubbed to powder. Their colour was a reddish white. Exposed to heat.

c. After

Treated with sulphuric acid.

c. After having poured on the residuum of experiment *b* an ounce of water, sulphuric acid was dropped in, till the residuum ceased to dissolve, even with the assistance of heat. A light brown residuum remained, weighing a quarter of a grain, which was oxide of manganese mixed with oxide of iron. The solution being evaporated to dryness, the salt was redissolved in water, and to the last yielded crystals of sulphate of magnesia; only half a grain of sulphate of lime was separated, amounting to a third of a grain of carbonate of lime.

Muriatic solution precipitated by ammonia.

d. The solution of experiment *a* having been treated as in experiment *A e*, yielded a precipitate, that weighed one grain after calcination, and consisted of alumine, containing traces of the oxides of iron and manganese.

This second variety then contains

Its component parts.

Magnesia	46·59
Carbonic acid	51
Alumine	1
Oxides of iron and manganese	0·25
Lime	0·16
Water	1
	100.

It comes very near the first, and differs only in some accidental matters.

3d variety.

C. The variety of magnesite, of which I am now proceeding to give the analysis, is perfectly white, more dense than either of the former, strongly adhesive to the tongue, and has a strong earthy smell. It has neither cavities nor marblings interiorly, but a few specks of silex. Care was taken to analyse only such pieces as contained none of these specks of chalcedony.

Treated with muriatic acid.

a. 100 grains of this magnesite, thrown in pieces into muriatic acid, lost 47 grs, without any heat being applied. A gelatinous residuum remained, which would not dissolve, though an excess of acid was added.

Exposed to a red heat.

b. 100 grs of pieces of this stone, exposed to a red heat for an hour, left 49 grs. The residuum was a little reddish, and

and dissolved gradually without effervescence in diluted sulphuric acid.

c. The solution of experiment *a* was carefully evaporated, and half an ounce of concentrated muriatic acid added with an equal quantity of water. This was boiled and filtered. There remained 4·5 grs of an earth, which readily dissolved in a caustic alkaline lixivium. This, with its insolubility in acids, showed it to be silex. Residuum of the muriatic solution.

d. The solution separated from the silex in the preceding experiment was supersaturated with ammonia, which rendered it slightly turbid. After the precipitate had subsided, the fluid was poured off. The precipitate, well washed and dried, weighed half a grain, and consisted of alumine, mixed with oxides of iron and manganese. The solution precipitated by ammonia,

e. The liquor of experiment *d* was decomposed at a boiling heat by carbonate of soda. The precipitate obtained, after it had been washed and heated redhot, weighed 45·5 grs. It was of a brownish colour. On redissolving it in sulphuric acid, a brown residuum remained, weighing half a grain, and composed of the oxides of iron and manganese. The sulphuric solution yielded sulphate of magnesia, from which a quarter of a grain of sulphate of lime was separated. and decomposed by carbonate of soda.

Of this variety of magnesite therefore 100 parts contain

Component parts.

Magnesia	45·42
Carbonic acid	47
Silex	4·50
Water	2
Alumine	0·50
Oxides of iron and manganese	0·50
Lime	0·08
	100.

VIII.

VIII.

METEOROLOGICAL JOURNAL.

1812.	Wind	Pressure. Max.	Min.	Med.	Temperature. Max.	Min.	Med.	Evap.	Rain	
2d Mo.										
Feb. 5	S E	29·58	29·54	29·560	47	41	44·0	—	0·59	☽
6	N W	29·86	29·54	29·700	47	35	41·0	—	1	
7	W	29·86	29·70	29·780	47	37	42·0	—	0·11	
8	N	29·96	29·86	29·910	41	36	38·5	·28	0·10	
9	N	29·98	29·95	29·965	43	38	40·5	—		
10	E	29·97	29·87	29·920	45	26	35·5	—		
11	E	29·87	29·60	29·735	48	33	40·5	—	2	
12	S	29·54	29·45	29·495	50	39	44·5	—	4	●
13	W	29·77	29·59	29·680	44	38	41·0	—	0·10	
14	Var.	29·65	29·48	29·565	48	39	43·5	—	0·24	
15	N W	29·69	29·65	29·670	47	41	44·0	·48	5	
16	N W	29·75	29·66	29·705	49	45	47·0	—	0·14	
17	W	29·80	29·46	29·630	50	40	45·0	—	2	
18	N W	30·06	29·80	29·930	46	38	42·0	—		
19	S	30·06	29·97	30·015	53	34	43·5	—		☾
20	S	29·97	29·84	29·905	54	42	48·0	—		
21	S	29·84	29·58	29·710	54	43	48·5	—	0·28	
22	Var.	29·59	29·55	29·570	50	41	45·5	·55	0·32	
23	N W	29·75	29·49	29·620	50	31	40·5	—	1·08	
24	N W	29·95	29·76	29·855	44	34	39·0	—		
25	S	29·40	29·30	29·350	44	32	38·0	—	0·12	
26	Var.	29·76	29·40	29·580	42	30	36·0	—		
27	Var.	29·76	29·70	29·730	50	31	40·5	—	6	○
28	S	29·70	29·65	29·675	47	31	39·0	—	1	
29	E	29·65	29·55	29·600	48	37	42·5	·67	3	
3d Mo.										
March 1	E	29·85	29·65	29·750	48	33	40·5	—		
2	N W	29·97	29·90	29·935	46	25	35·5	—	1	
3	E	29·90	29·86	29·880	44	38	41·0	—	0·22	
4	S W	29·87	29·80	29·835	52	35	43·5	—	3	
5	Var.	30·04	29·75	29·895	47	36	41·5	·30	0·13	
		30·06	29·30	29.738	54	25	41·73	2·28	3·71	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES.

NOTES.

Second Month. 8. Rainy evening. 10. p. m. clear: a fine blush in the evening twilight. 11. Hoar frost. 12. Stormy night. 14. Rainy morning: very stormy day. 16. Wind boisterous all night, with rain. 17. Stormy night. 20. A very fine day: lunar halo at night. 21. Cloudy: a heavy shower of hail about half past 9, p. m.: night stormy. 22. About 9 a. m. came on a great storm of wind and rain, mixed with hail, which continued about an hour: on its ceasing, the clouds dispersed and the wind changed to W. About noon it became again stormy, continuing so at intervals till half past four, when it began to hail with great violence; this was followed by rain, and during the storm there were frequent flashes of lightning and some distant thunder. 23. Cloudy: a large lunar halo: wind high in the night with rain. 24. Very stormy morning: heavy rain about 3 a. m. with the wind very strong from N. W. In an hour after, snow and sleet, with a freezing air: clear evening: the moon bright. 25. Very stormy. 27. 28. Hoar frost. 29. Misty morning.

Third Month. 2. Fine, with occasional clouds. 3. Hoar frost: night rainy. 4. Wet morning. 5. Wet night.

RESULTS.

Winds variable.

Barometer: highest observation 30·06 inches; lowest 29·30 inches; Mean of the period 29·738 inches.

Thermometer: highest observation 54°; lowest 25°; Mean of the period 41·73°.

Evaporation 2·28 inches. Rain, &c. 3·71 inches.

For the chief part of the observations in the present period, I am again indebted to my friend John Gibson.

LONDON, L. HOWARD.
Third Month, 23, 1812.

IX.

Notice respecting the Geological Structure of the Vicinity of Dublin; with an Account of some rare Minerals found in Ireland. By WILLIAM FITTON, *M. D.* *Communicated by* L. HORNER, *Esq. Sec. to the Geological Society**.

Object of the paper.

THE following observations are to be ascribed principally to the late Rev. Walter Stephens. I present them to the Geological Society in their present imperfect form, with the hope that they may attract the attention of mineralogists to the country in the vicinity of Dublin; for they are sufficient to show, that very interesting information may be expected from a correct examination of that district; which from its situation is easy of access, and presents many advantages to the observer. I shall subjoin to a brief statement respecting the geological structure of that country, an account of some minerals of not very common occurrence, recently found in Ireland.

The country described.

The city of Dublin is placed in a flat limestone country, at the distance of about five miles to the northward of a range of mountains, which form the verge of a mountainous district, extending thence for more than thirty miles to the southward. Through this tract there passes, in a south-western direction from the shore on the south side of Dublin bay, a broad body of granite, bounded on its eastern and western sides by incumbent rocks of great variety; the structure and relations of which, as well as of the granite mass, are in many places very distinctly exhibited.

Mines.

Within this mountainous district, distinguished by the interesting and beautiful scenery which it presents, are found the copper mines of Cronebane and Ballymurtagh†; and the lead mines of Glenmalúr; the veins of lead ore at Dalkey, and that near the Scalp also belong to it. The stream works commonly called the Gold mine, at the mountain Croghan

* Transactions of the Geological Society, vol. I, p. 269.

† An account of the metalliferous waters of these mines was published in the Philosophical Transactions so far back as the year 1752, vols. XLVII, and XLVIII.

Kinshela,

Kinshela, are on the southern range of this district and of the county of Wicklow; and gold has been found within it, at another mountain also named Croghan, about seven miles to the northward of that place*.

The occurrence of tinstone at the "Gold-mine", where it has been obtained in fragments†, is a fact which deserves attention; for from the great extent of primitive country in the Wicklow mountains, the probability of finding veins of tinstone there are considerable. Porcelain earth in purity equal to the "China clay" of Cornwall has been found in the lands of Kilranelagh, on the south-western side of this county; and granite in a state of decomposition is found so extensively in other parts of it, that this valuable production may very probably be obtained there in considerable quantity. Tinstone. Porcelain earth.

The country around the village of Bray, at the distance of ten miles from Dublin, presents within a small space an instructive series of rocks; and the appearances observable at Killiney, first noticed I believe by Dr. Blake of Dublin, particularly deserve attention. Schistose beds are to be seen at that place to a considerable extent reposing upon granite; and the line of junction, which begins here at the seaside, may be traced by the eye for some miles across the country. The regularity of this junction is remarkable on the top of Rochestown hill, adjoining that of Killiney; where ledges of granite, against the foot of which the incumbent rocks incline, present in several places a rectilinear course for many fathoms together. On the shore at the base of Killiney hill, the granite is traversed by numerous veins, many of which themselves consist of granite; and in some instances two granite veins, differing from each other and from the mass in fineness of grain and in proportion of their ingredients, are seen to intersect; one vein often deranging the continuity of the other's direction. The substance of these veins is perfectly continuous with that of the mass through which they run, and the surface of the fracture passes through both without interruption. Beds of schist upon granite.

* Gold is said to have been found also in the King's River, near the village of Holywood, in the county of Wicklow.

† Report by Messrs. Mills and Weaver. Trans. Dublin Society.

The

Sugarloaf mountains.

The conical masses of the Sugar-loaf mountains, with the summits of Brayhead, and Shankhill, resembling them in structure, are composed of quartz; and it may be remarked, that the conical form appears to be in some measure characteristic of mountains composed of this substance; for Mr. Jameson informs me, that he has seen in Lusatia detached conical summits composed of it; and that the well known Paps of Jura, and the conical summits in the mountains separating Caithness from Sutherland, are of the same material; as also is, according to Dr. Berger, the mountain Durnhill, near the town of Portsoy*.

Contact of granite with incumbent rocks.

The actual contact of granite with incumbent rocks has been observed at the following places in the counties of Dublin and Wicklow. On the western side of the granite, in a streamlet joining the Dodder, west of the glen above Billinascorney; at Golden hill, near the granite quarries; and at Kilranelagh: on the eastern side, at Killiney; at the southern extremity of the Scalp; at Tonelagee; near Aghavanagh to the eastward; and at the south-western side of Croghan Kinshela. On the shore of Dublin bay, between Booterstown and Blackrock, a mass of compact limestone is visible within a few fathoms of the granite, but in the interval the rock is concealed.

Rocks of the trap family.

Near Ballinascorney, on the western verge of the granitic mountains nearest to Dublin, rocks of the trap family occur; and thence to the south-westward, along the borders of the counties of Wicklow and Kildare, various intermediate rocks between the granitic tract above mentioned and the limestone of the flat country to the westward will be found. At Arklow rock, on the south-eastern extremity of the county of Wicklow, columnar rocks of the trap family have been observed by Dr. Wollaston and the Rev. Dr. Brinkley.

Varieties of limestone.

The quarries in the more immediate neighbourhood of the city afford many varieties of calcareous productions. The

Vast thickness of quartz.

* Humboldt states, that in South America, quartz constitutes, exclusively, a mass of more than nine thousand five hundred feet in thickness, which he considers as of a "formation" peculiar to the Andes. He has not mentioned the form of the summits. Tableau Phys. p. 128.

calp

calp of Mr. Kirwan, a variety of limestone, of which an excellent description and analysis have been published by Mr. Knox*, is the prevailing rock. Brown-spar (Jameson) is found in veins at the quarries near Dolphinsbarn; and beds of magnesian limestone were observed by Mr. Stephens in the bed of the river Dodder, at Miltown, and at Classons-bridge, above that place. The petrifactions, which abound in many parts of this limestone country, the calp, and the beds of magnesian limestone afford some of the features which may assist in deciding on the "formation" of Werner, to which it is to be referred; a point of considerable interest, from the great extent which the limestone occupies in the counties of Dublin, Kildare, and Carlow.

In the peninsula of Howth, which forms the northern side of Dublin bay, gray ore of manganese with brown iron-stone, and brown iron-ore (Museum of Dublin College, Nos. 1067-8, 887.) have been obtained in considerable quantity: and a variety of the earthy black cobalt ore of Werner has been found by Mr. Stephens and Dr. Stokes on the southern side of the hill, forming a crust of a rich blue colour lining the fissures of a rock of slate clay nearly approaching to whet-slate, (Mus. T. C. D. No. 267): Mr. Tennant has in this substance ascertained the presence of the oxides of cobalt and of manganese; and the discovery of it is important, as it indicates the probability of the existence of other more valuable ores of cobalt in that neighbourhood. Lugnaquilla, which is supposed to be the highest of the Wicklow mountains, is situate to the south-westward of the centre of the mountainous district: I have found it, by the barometer, to be 2455·1 feet above the house of Mr. Greene at Kilranelagh, which is itself considerably elevated above the sea. Cadeen, a hill detached from the body of the mountains, and forming a striking object from the adjacent flat country, is 1558.9 feet; Baltinglass hill, 681.8 feet; Eadestown, 749·4 feet; Brusselstown, 740.1 feet; Kilranelagh hill, 705·5 feet above the same place†. Of

Ores. Heights.

* Transactions of the Roy. Irish Acad. vol. VIII, p. 207.

† The first three heights above mentioned are each the mean of three observations, the rest are from single observations, with two excellent barometers. Mr. Greene's house is (by a single observation) 95·08 feet above

Mountains nearest Dublin.

Of the mountains nearest to Dublin, one of the highest, Garrycastle, is 1531.7 feet above the level of the road at Ballinteer; and the Three Rock mountain is 1247.9 feet above the same place, the elevation of which is considerable. The highest point of Howth is 567 feet above high-water mark.

Account of Minerals, &c.

Vesuvian.

1. *Vesuvian.*—(*Idocrase*, Haüy). This substance was observed by Mr. Stephens in specimens found by me at Kilranelagh, where it occurs in irregular crystalline masses, in a rock composed of common garnet of a reddish-brown colour, of quartz for the most part greenish, apparently from the admixture of a lamellar fossil of that colour, and a small quantity of felspar. The crystalline form of the garnet is here often very distinct, but in the specimens hitherto found, that of the Vesuvian is not well exhibited, although some indistinct prisms are to be observed. In general, its particles assume a scapiform aggregation, sometimes approaching to stellular, a form which I have not observed in specimens of this substance from other places; but its fusibility, lustre, colour, and other characters leave no doubt as to its nature.

The blocks of this compound at Kilranelagh were not in their natural place, but their size, their great weight and singular form render it probable, that they were not far removed from it. Garnet rock is described as occurring in beds in primitive mountains, and the country at Kilranelagh is of this description.

It is remarkable, that a compound much resembling that which I have described occurs also in the county of Donegal, whence specimens now in the cabinet of the Dublin Society, and that of Dublin College, No. 30, were obtained. The garnet and vesuvian in these specimens are scarcely to be distinguished from those of Kilranelagh; and, as at that place, are accompanied by quartz, often of a simi-

above the level of the cross roads at the bridge of Tuckmill, a little village on the river Slaney; the elevation of which above the sea will be very well supplied when the line of the grand canal shall be extended in this direction, as is now intended.

lar

lar greenish colour; with the addition however of bluish gray granular limestone, and a fibrous substance, not improbably tremolite, mixed with carbonate of lime. I have not seen any felspar in the specimens from Donegal*.

2. Grenatite. (Staurotite, Haüy). This was detected by Mr. Stephens in crystals in a micaceous compound of which I found a specimen at the Glenmalur lead mines in the county of Wicklow; the crystals are small, but their colour, form, and characteristic crossing are very distinct, and they are infusible before the blowpipe. Grenatite.

3. Beryl. (Var. of Emerald, Haüy). The precious beryl has been found by Mr. Stephens and myself imbedded in granite, near Lough Bray in the county of Wicklow. (Museum of Dublin College, No. 39.) Mr. Weaver has discovered it in blocks of granite, near Cronebane in the same county; and I have found in the Dublin mountains, above Dundrum, specimens probably belonging to the same species. Beryl.

4. Andalusite. (Feldspath apyre, Haüy). This has been found by Mr. Stephens and myself, in very distinct specimens, on the north east side of Douce mountain in the county of Wicklow, apparently imbedded in the mica slate of which that mountain is composed, and accompanied by quartz, mica, and a remarkable crystallized substance hereafter to be mentioned. It differs from the andalusite of Spain and of Scotland chiefly by inferior hardness; for although some pieces scratch window-glass, others yield easily to the knife: but the Count de Bournon has observed an equal variation in the hardness of specimens of this substance found by him at Forez†; and I have found that of the Scottish stone to vary very much. Andalusite.

This fossil seems to have been first taken notice of under the name of wurflicher (cubic) felspath by Karsten, who took his description from specimens in the Leskean cabinet now in Dublin‡ (No. 907 *b*, &c.); and from a comparison of

* Since this paper was written, I have found that this compound from Donegal has been described by Mr. Sowerby. British Mineralogy, August, 1810, p. 133.

† Journal de Physique, XXXIV, p. 453. 1789.

‡ Bergmann's Journal, vol. II, p. 809. Ann. 1788.

of these with the specimens from Douce, the identity of Karsten's fossil with andalusite is ascertained. I have not found however, that this claim to the first detection of it has been mentioned by subsequent writers: although his opinion with respect to its affinity to felspar accords with that which Haüy is disposed to adopt. Tableau comparatif, &c. p. 217.

To this species is also to be referred a mineral which occurs in great abundance at Killiney in the county of Dublin, first observed there by Dr. Blake, and for some time considered as belonging to a nondescript species. It is most remarkable on the shore at the southern extremity of the cliff under the obelisk hill, where it appears thickly on the surface of beds of mica slate; and it seems to abound also imbedded in the substance of that rock, although less distinctly visible until it has been exposed to decomposition, being less affected by exposure than the rock in which it is contained.

The andalusite, when thus brought to view, appears generally in slender prismatic crystalline pieces rounded at the angles, seldom sharp, promiscuously aggregated, sometimes in a stellular form, and of a grayish-black colour, remarkably contrasted with the lustre and light colour of the micaceous substance in which they appear. But in fresher pieces, the form, colour, cleavage, and other characters of this mineral are distinct; and I have observed an approach to the peculiar appearances, which it presents at this place, in some Spanish specimens, where the crystalline shoots had assumed a scapiform arrangement.

Crystals of indurated talc?

5. The andalusite of Douce mountain is accompanied, as has been mentioned, by a crystallized mineral, the characters of which have much affinity to those of indurated talc; and which is placed under that denomination in the collection of Dublin College (Nos. 405, 6, 7); and a specimen of the same kind, stated to be from Glendalagh in the county of Wicklow, was found in the same collection (No. 404.)

The crystals are rhomboidal prisms, of which the length is in some instances more than twice the breadth, but no acumination is observable. They are easily cut by the knife,

knife, faintly translucent, their colour yellowish-gray. Small fragments before the blowpipe appear to swell a little from the separation of the folia on the first application of the heat; they become white, and give with some difficulty a solid white enamel. The specimens to which I have access at present do not enable me to give any detail of the remaining characters.

The connection of this substance with the andalusite of Douce is remarkable; the latter often forming the nucleus of crystals externally of four sides, sometimes filling nearly the whole of the interior; but in other specimens forming little more than an axis, with rounded edges, and of irregular form, from which the folia of the investing talclike substance appear to radiate.

The occurrence of indurated talc in crystals has hitherto been very rare: it is not mentioned by Jameson; and Brochant, though he quotes from Emmerling the rhomboidal prism as one of its forms, expresses doubt as to the correctness of the statement; I therefore do not give that name to the crystals found at Douce, without some uncertainty.

6. Hollowspar, Jameson. (Macle, Haüy). Very distinct specimens of this mineral have been found by Mr. Davy, at Aghavanagh in the county of Wicklow; and I have observed it at Baltinglass hill, within a few miles of that place. I may mention here, that from the appearances of many specimens found in the neighbourhood of Killiney, Mr. Stephens was inclined to suppose, that a connection existed between this singular species and andalusite. Hollowspar.

7. Pitchstone. This substance is found in a vein traversing granite, in the vicinity of Newry in the county of Down. I am indebted to Mr. Jameson of Edinburgh for much of the following description of its external characters, as it appears there. Pitchstone.

Its colour is intermediate between mountain and leek green. It is massive. Fracture small and not very perfect conchoidal.

Internal lustre, resino-vitreous and shining. It exhibits lamellar distinct concretions; the plates are from one fourth to one tenth of an inch in thickness, and are farther divisible into pieces of the rhomboidal form of various angles.

The

Pitchstone. The surface of the concretions is smooth, and strongly glistening. Slightly translucent on the edges. It scratches window glass, but is easily scratched by quartz. Easily broken. Specific gravity, 2·29. Before the blowpipe without addition it yields a grayish white frothy enamel.

It is in some places porphyritic, containing imbedded minute crystals of feldspar and of quartz.

A letter from a very intelligent observer, who has examined this substance in its native place, states the following particulars respecting its position.

" The vein is first observable in the Townland of Newry, " at the bottom of a bank of granite, about half a mile from " the northern end of the town, on the right of the road " leading to Down Patrick. It crosses the road, and runs " due westward, ending on the side of the great road from " Newry to Belfast. Its length, so far as hitherto observed, " is half a mile.

" The rock, which is covered with mould to the depth of " about a foot, consists of a gray granite. The vein is " about two feet and a half, or two and a quarter in width; " at the places of contact both the granite and pitchstone " are disintegrated, the latter being almost as soft as clay, " but becoming gradually harder, as it approaches the " centre of the vein. The structure of the vein is foliated, " the folia being perpendicular to the horizon, and also to " the walls; and beside these there are seams, that run " longitudinally, parallel to the horizon, and nearly per-" pendicular to the folia."

Although this substance presents some peculiarity, in being divisible into rhomboidal fragments, it approaches in this respect to the pitchstone of Arran (in lamellar concretions) which holds as it were a middle place between it, and that possessing the more usual characters.

Mr. Jameson has described a vein of pitchstone " running in granite," observed by himself in Arran*; and he states, that " lamellar distinct concretions have been hitherto ob-" served in the pitchstone of that island only.†"

* Min. of Scottish Isles, 4to, vol. 1, p. 81.

† Jameson's Mineralogy, vol. I, p. 261.

8. The

8. The granular sulphate of barytes, hitherto very rare, has been found, as the Rev. Mr. Hincks of Cork informs me, by Dr. Wood of that city, on the seashore, near Clonakilty, whence a specimen in the Museum of Dublin College, (No. 653) has probably been obtained: it is accompanied by iron pyrites. Granular sulphate of barytes.

9. Wavellite. This remarkable mineral has recently been found in the county of Cork, at Springhill near Tractou Abbey, about ten miles south-eastward from the city. The Rev. Mr. Hincks of the Cork Institution, from whom the specimens that I have seen were obtained, informs me, that it was found at a small distance from the surface, near the base of a hill composed of flinty slate, and that he has seen it adhering to a piece of rock of that description. But it has occurred principally detached in the form of globular nodules, irregularly grouped together, and of various sizes, the longest about an inch in diameter, externally coated with a yellowish brown earthy crust, and within composed of radiating crystalline spiculæ, the characters of which agree very nearly with those of the wavellite from Devonshire, described by Mr. Davy; indeed some of the specimens from the county of Cork are scarcely to be distinguished from some of those obtained at that place. Wavellite.

The most distinct specimen, that I have seen, was a nodule about three fourths of an inch in diameter, in part affected by decomposition, and containing some small spongy cavities. On its external surface indistinct dihedral terminations of the crystalline shoots are discernible; and internally, where it is not decomposed, its lustre is higher and more glassy than is common in the Devonshire fossil. The specific gravity of that part of it, which was very pure and nearly transparent, was 2·34.

The nodules are in some instances decomposed throughout; the spiculæ, having lost their lustre, acquire a dull gray or brownish colour, and become much softer than when unchanged; and Mr. Hincks has seen some of them altogether in the state of clay, apparently from the effect of decomposition.

It would appear that the fluoric acid, of which Mr. Davy has ascertained the presence in the wavellite from Devonshire

shire, exists also in that from Cork: for glass is corroded by heating upon it, in a drop of sulphuric acid, a fragment of the mineral from either of those places.

X.

On the Native Country of the Solanum tuberosum, or Potato. By BENJAMIN SMITH BARTON, *M. D., Mem. of the Am. Phil. Soc. &c.* Communicated by JOHN MASON GOOD, *Esq., F. R. S. Mem. of the Am. Phil. Soc. and F. L. S. of Philadelphia.*

On the native country of the potato.

IN the *Transactions of the Horticultural Society of London*, there is a paper, by sir Joseph Banks, on the native country of the solanum tuberosum, or potato*. I have read this paper, with much satisfaction: but as my opinions on some of the points relative to this question are essentially different from those of the learned and excellent baronet, I have drawn up, without much regard to order, the following memoir, which I beg leave to communicate to the public, through my candid and learned friend, Mr. John Mason Good.

The potato supposed to have been introduced from Virginia:

Sir Joseph Banks thinks it very certain, that the potato, though not exclusively a native of Virginia, was actually imported from that part of the American continent into Europe: and that the seed potato of the English was introduced exclusively perhaps into Britain and Ireland from Virginia.

but it is not a native of North America.

It is my decided opinion, that the potato is not a native of Virginia, or of any other part of the North American continent: that it was not even known, in a *cultivated* state, in any of these more northern latitudes of America: and by consequence, that it was not imported into Europe from these regions of the new world.

* An Attempt to ascertain the Time when the Potato (Solanum tuberosum) was first introduced into the United Kingdom, &c., by the Rt. Hon. sir Joseph Banks, bart. K. B., P. R. S. &c. Transactions of the Horticultural Society of London, vol. I, part I, art. ii, London: 1807. [See Journal, vol. XX, p. 1.]

I shall

I shall here examine the grounds, upon which the opinion of sir Joseph is founded. Ground of sir J. Banks's opinion.

"The potato now in use (solanum tuberosum) was brought to England by the colonists sent out by sir Walter Raleigh, under the authority of his patent, granted by queen Elizabeth, 'for discovering and planting new countries, not possessed by christians,' which passed the great seal in 1584. Some of sir Walter's ships sailed in the same year; others, on board of which was Thomas Herriot, afterward known as a mathematician, in 1585; the whole, however, returned, and probably brought with them the potato, on the 27th of July, 1586."

Sir Joseph continues: "This Mr. Thomas Herriot, who was probably sent out to explore the country, and report to his employers the nature and produce of its soil, wrote an account of it, which is printed in De Bry's Collection of Voyages, vol. 1. In this account, under the article of roots, p. 17, he describes a plant called openawk. 'These roots,' says he, 'are round, some as large as a walnut, others much larger; they grow in damp soil, many hanging together, as if fixed on ropes; they are good food, either boiled or roasted.'"

Sir Joseph adds, that "in the Herbal of Gerard, which was published in 1597, there is a figure of the potato, under the name of potato of Virginia; and that this writer tells us, that he received the roots from Virginia, otherwise called Norembega."

I shall now examine the different arguments, which sir Joseph has adduced to prove, that the potato was really indigenous in Virginia, in the order in which he has mentioned them. The arguments examined.

1. He thinks it probable, that the potato was brought home by sir Walter Raleigh's men, in the month of July, 1586. We have here, however, no proof whatever, that the root in question was brought into Britain at this time, and in particular, that it was brought from Virginia. But sir Joseph assumes it as a fact, that the plant, which Mr. Herriot met with in Virginia, and which he calls *openawk*, is no other than the solanum tuberosum. Nor will it be denied, that the description of the openawk, so far as its roots are The openawk brought from Virginia, not the potato.

 concerned,

concerned, applies pretty well to our potato. Yet I think it very certain, that they are very different plants: and of this the learned baronet would himself have been convinced, if he had consulted a figure of the openawk. Whether the plant is figured in De Bry's Collection of Voyages, to which sir Joseph refers, I know not, as I have not an opportunity of consulting that valuable work at present.

Some of the Indians call the potato now hob-be-nac: but this of little weight,

The opinion, that the openawk of Mr. Herriot is the potato, will, no doubt, at first sight, be thought to derive some weight from this circumstance, that, to this day, some of our Indians call the potato (solanum tuberosum) *hob-be-nac*. But I apprehend that this circumstance is, really, of little consequence in the investigation of the subject: for the same Indians (the Lenni-lennápe, or Delawares) denominate the turnip, which is unquestionably a foreign vegetable, *hob-be-nis*: and others of our Indians call the glycine apios, which is soon to be more particularly mentioned, *hopnis*, or *hapnis*. Moreover, the common name, at this time, in the vicinity of Philadelphia, for the sagittaria sagittifolia, or common arrow-head, the root of which is eaten by the Indians, is *hob-ne*, or *hub-ne*, which is, doubtless, a corruption of the Indian name. It is probable, therefore, that the meaning of all these varieties of a common word is nothing more than "esculent root," or something of the kind: in the same manner as *tuckahoe*, or *tuc-ca-ho*, is the name, in the language of other Indians, for several very different species, and even genera, of plants, the roots of which were eaten frequently in the shape of bread, by the Indians.

as it simply means esculent root.

The openawk figured by John de Laet.

I have just said, that I have not an opportunity of consulting the work of De Bry. But I regret this circumstance the less, since John De Laet, in his very valuable work the *Novus Orbis*, has given us some copious extracts from the commentaries of Herriot; and among other plants, or parts of plants, which he describes from that very respectable traveller and mathematician, the Flemish historian has furnished us with a description together with a figure of the openawk.

After speaking of the mays, the papaw (annona triloba, *Linn.*), and some others, De Laet says, "Præter has et alias herbas, etiam radices edules sponte hic (in Virginia) prove-

proveniunt; imprimis quas indigenæ *openawk* vocant, rotundæ, juglandi nucis magnitudinis pares, interdum et multo majores; nascuntur humidis et paludosis locis, plures inter se coherentes et veluti funiculo colligatæ; in aqua coctæ, aut igne tostæ, boni sunt alimenti*."

The openawk, however its roots may seem to resemble that of the potato, must be, I say, a very different plant. A bare reference to the figure in De Laet will be sufficient to show the validity of this assertion. Indeed it has not the most distant resemblance to the potato. Instead of the pinnated leaves of the latter, the openawk has simple ovate leaves. Neither do the places of growth of the two plants agree very well. We are told, that the openawk grows in moist and marshy situations. In such situations who ever thought of planting the potato? and so far as we know any thing of the soil of the latter in Chili, where, if it be not truly indigenous, it has, at least, been most anciently known, it is never found in marshy soil, but in a soil of a very different kind: in the fields and upon the mountains. It is true, however, that De Bry, according to sir Joseph Banks, places the openawk merely in a "damp soil."

It differs from the potato in its leaves and place of growth.

It may be asked, why place so much reliance upon the figure of the openawk, in the work of De Laet? I answer, that many of the figures of vegetables, animals, &c., in the *Novus Orbis*, though merely cut upon wood, are far from being inaccurate representations of the objects they are intended for. Linnæus, Willdenow, and other naturalists have not been ashamed to refer to some of De Laet's icons of plants.—See in the *Species Plantaum* polygonum sagittatum.—But, I repeat it, it is sufficient to cast the most superficial glance upon the wooden cut of openawk, to be fully satisfied, that it could never have been intended to represent the solanum tuberosum.

De Laet's figures and representations.

I wish it were as easy to determine, what plant the openawk is, as what it is not. The description of the root answers pretty exactly to that of the glycine apios of Linnæus, a very

The root of the openawk of Herriot answers to that of glycine apios,

* Novus Orbis, seu Descriptionis Indiæ occidentalis, Libri XVIII. Authore Joanne de Laet Antverp., lib. iii, cap. XXII, p. 90. Ludg., Batav. 1633.

a very common plant in many parts of North America, and perhaps no where more common than in Virginia. This fine plant grows in moist situations, in a rich soil, as along the banks of our rivers, &c. It is well known by the name of Indian potato, wild potato, earth nuts, ground nuts, &c. The root is so abundant, and so well tasted, that the plant is worthy of being cultivated; and the more so, as even its seed, or "pease" as they are sometimes called, when boiled, are deemed a delicacy at the table.

which is a plant well deserving culture:

But De Laet's figure not like it.

But De Laet's openawk is as unlike glycine apios, as it is unlike solanum tuberosum. Indeed, I must dismiss this part of my subject by candidly confessing, that I know not what plant the openawk is. Perhaps, it will be found, in the course of farther inquiry, to be a species of one of the three genera, arum, pothos, or sagittaria.

Other esculent plants mentioned by de Laet.

Beside the openawk, De Laet speaks of, 1. the *okeepenauk*. This is, unquestionably, the vast tuber, mentioned by Clayton (*Flora Virginica*, p. 176), which I call tuber tucca. 2. The *kaistucpenauck*, which has a white root, "ovi gallinæ forma et magnitudine." This I take to be a sagittaria. 3. *Tsinaw*, a climbing plant, of which bread is made, is I suspect, a species of smilax. 4. *Coscushaw*. Of the root of this also the Indians made bread. The plant grows in moist and stagnant places. The recent juice is poisonous and must be expressed before the pulpy and fibrous part can be made into bread. This, certainly, is not solanum tuberosum, but, if I mistake not, a species of arum. I take it to be Captain Smith's *tockawhoughe*. 5. *Habascon*, "a hot root, of the shape and size of the parsnip": perhaps the root of some species of angelica.

Gerard's testimony of little weight.

II. I am sorry that I have not an opportunity of consulting the *herbal* of Gerard. But I readily take it for granted, that what this old writer has said relative to the potato is correctly stated by sir Joseph. Allowing this to be the case, the statement is not of very material importance in the present inquiry. Gerard may have meant nothing more, than that the plant was said to have come from Virginia, or Norembega. Every botanist knows how vaguely, or erroneously, the native countries of many vegetables are mentioned, even in some of the best and most classical works on the

Habitats of plants frequently given erroneously.

the science. Have not the plants of Canada, New York, and Pennsylvania, been asserted to grow—perhaps exclusively—among the toupinamboes of Brazil? Even Linnæus, speaking of aconitum uncinatum, says "Habitat in Philadelphia." I presume, that he intended to say, Pennsylvania, though I think the plant has not yet been found wild in any part of this state. Hardly any part of botany stands more in need of reform than that which relates to the *habitats* of vegetables. Zoological science, too, in this respect, may be greatly improved and corrected.

Zoology deficient in this respect.

And where was Norembega? I believe the geographers would find some difficulty in determining this point. In De Laet's map of "Nova Anglia, Novum Belgium, et Virginia," we find the word "Norembegua" laid down far to the north of the most northern limits of modern Virginia; somewhere about latitude 45! The historian tells us, that he is at a loss to determine the situation of the celebrated city and river of Norembegua, concerning which many fables have been written. He conjectures, however, that the river of this name is that called by the English "Pennobscot." "Qui superioribus annis de hisce regionibus scripserunt, multa fabulati sunt de celebri oppido et flumine *Norumbegua*," &c. Lib. ii, cap. XVIII, p. 55.

Where was Norembega?

That the potato was not brought from Virginia, and that it is not even a native of that part of North America, will, I think, appear more than probable from this striking circumstance, that not one of the earlier visitors or describers of the country has mentioned this vegetable, in their lists of those which they found, either wild or cultivated, among the Indians. In Mr. Herriot's account there is no reference to any thing like the potato, with the exception of the openawk, and a few other esculent roots, the aboriginal names of which I have already mentioned. And although I may not be able to say confidently what are the precise species of plants called openawk, kaistucpenauck, tsinaw, coscushaw, &c., I think I have been successful in proving, that the solanum tuberosum is neither of them.

The potato not a native of Virginia.

The silence of Herriot, in regard to the potato, is with me a circumstance of considerable weight. For this writer was no common observer. He seems to have examined, with nice

Herriot, an acute observer, does not mention the potato.

nice and philosophic attention, the manners, the customs, &c., and to have paid particular attention to the dietetic articles of the Indians. Had he found the solanum tuberosum in Virginia, either as cultivated by these rude people, or as growing wild in their woods, &c., he would not have neglected to give us some information on the subject.

The potato not mentioned as a plant of Virginia by Smith.

Nor is the potato mentioned as one of the *indigenous* plants of Virginia by the famous Captain John Smith, who came into that country in the very first years of the 17th century, and who resided there a long time: certainly long enough to have made himself acquainted with a vegetable of such primary importance to the colony, if it had really existed there; and especially if it had been cultivated by the Indians. It is true, that Smith does make some mention of the potato: and I shall afterward avail myself of what he has said on the subject, as one of the most powerful arguments in support of my opinion, that the potato was entirely unknown in Virginia, when the first English colonists took possession of the country.

The potato not mentioned as a plant of Virginia by any good authority but Mr. Jefferson;

In truth, I do not find, that this vegetable is mentioned as a native of Virginia, or as cultivated in this part of the continent when it was first discovered, by any writer who had enjoyed good opportunity of obtaining precise information on the subject, except by Mr. Jefferson, the learned president of the American Philosophical Society.

and supposed by him to have come from the south.

This gentleman mentions the potato among the vegetables which were found in Virginia when first visited by the English; but (he adds) "it is not said whether of spontaneous growth, or by cultivation only. Most probably they were natives of more southern climates, and handed along the continent from one nation to another of savages*.

His authority for its being known early there not mentioned.

I know not from what source of authority Mr. Jefferson obtained the fact, that the potato was found in Virginia, when first visited by the English. It is not altogether unlikely, that my illustrious friend was misled by the same passage in Herriot, which misled sir Joseph Banks: by the short and imperfect description of the unknown openawk. Mr. Jefferson did not obtain his information from Mr. Bever-

* Notes on the State of Virginia. Original edition, page 68.

ley,

ley, a respectable writer, who published an interesting little work on the history of Virginia early in the sixth century.

Mr. Beverley. Mr. Beverley could not find the solanum tuberosum in Virginia. He says, indeed, that the native Indians "had originally amongst them Indian corn, pease, beans, potatoes, and tobacco." But he afterward gives a more particular account of their potatoes. "Their potatoes (he says) are either red or white, about as long as a boy's leg, and sometimes as long and big as both the leg and thigh of a young child, and very much resembling it in shape. I take these kinds to be the same with those, which are represented in the herbals to be Spanish potatoes. I am sure those called *English* or *Irish* potatoes are nothing like these, either in shape, colour, or taste*".

His account of the Indian potato.

This is, certainly, an important passage. It almost proves, what I hope to render quite certain in the sequel, that the Indians of Virginia were entirely unacquainted with the solanum tuberosum, when these people were first visited by the Europeans. The long potatoes, which Mr. Beverley mentions, are, certainly, varieties of the convolvulus batatas, well known in the United States by the name of "sweet potato." But it is a fact, that beside this valuable plant, which the Indians of Virginia, &c., have, for a long time, cultivated, these people ate, though I think they did not cultivate, another species of the same genus, the convolvulus panduratus, which is still known in some parts of the United States, by the name of "Indian potato."

This a variety of the convolvulus batatas.

Descending farther south into Carolina, we cannot discover, that the Indians of that great tract of country possessed as a native, or cultivated as a foreign, plant, the solanum tuberosum, *before* their intercourse with the Europeans. Mr. Lawson, who resided in Carolina, in the very first years of the 18th century, mentions potatoes as some of the "garden roots," that thrive well in Carolina†. He does not

The potato not known in Carolina till introduced by Europeans.

Lawson.

* The history of Virginia, in four parts. By a native and inhabitant of the place. Pages 125, 127. London: 1722. The second edition.

† The garden roots, that thrive well in Carolina, are carrots, leeks, parsneps, turneps, potatoes of several delicate sorts, ground artichokes," &c. A new voyage to Carolina, &c., p. 77. London: 1709. 4to.

not mention them in his list of the indigenous plants of the country; and I am led to believe, from his manner of expressing himself, that they were not cultivated by the Indians. It is to be observed, however, that this intelligent traveller mentions the Indian names, among two of the tribes, or nations, of the potato. This, however, from what I have already said, is no manner of proof, that the solanum tuberosum was really an indigenous plant in North Carolina, where Lawson made his principal observations and inquiries. Every one, acquainted with the Indians, has been struck with the quickness of their mental perceptions, and with the rapid ease with which they bestow names, often very significant, upon new objects, especially natural objects, which they have never seen before*.

Its name in Indian tribes mentioned by him.

The Indians very apt at giving names.

Colonel Hawkins says,

My friend, Colonel Benjamin Hawkins, who resides as public agent from the government of the United States among the Creek Indians; and who is, perhaps, as well acquainted with the history, manners, state of improvement, &c., of these and other southern tribes of savages, as any man in America; assures me, that all the Indians, with whom he is acquainted, agree in considering the solanum tuberosum as a foreign or strange plant in their countries; and that it is only within a very few years, that these people have begun to pay any attention to the cultivation of this plant, which they explicitly say they received from the European Americans, or whites.

that all the Indians ascribe the introduction of the potato to the whites.

Bartram's testimony.

Mr. William Bartram (MS. *penes me*, speaking of the southern Indians of Carolina and Georgia) says, "Their vegetable food consists chiefly of corn (zea), rice, convolvulus batatas, or those nourishing roots usually called sweet or Spanish potatoes; (but in the Creek confederacy, they never plant or eat the solanum tuberosum, or Irish potato, vulgo.")

Other similar arguments

I might, without difficulty, go on to adduce many other proofs, or arguments, similar to those already mention-

* The Tuscaroras and the Woccous, the two Indian tribes mentioned by Lawson, call potatoes, *untone* and *wauk*. These tribes had much communication with the English, at an early period.

ed,

ed, all tending to establish my position, that the solanum tuberosum was not found, *either in a wild or in a cultivated state*, in Virginia, or in any of the adjacent countries of North America, by the first discoverers and colonists of these portions of the new world. But it is time, perhaps, to try the question, which we are examining, by another set of arguments. might be adduced.

I have already said, that Captain John Smith does not mention the potato among the "indigenous" plants of Virginia. But this gentleman is not wholly silent on the subject of our plant. On the contrary, his *Historie* contains a memorable passage, which seems to have escaped the notice of sir Joseph Banks, Mr. Jefferson, Mr. Willdenow, baron Humboldt, and all the other writers, who have contended, that the valuable esculent plant, of which we are speaking, was originally found in Virginia: a passage from which it is safe to infer, at least, thus much, *that the potato was not known by the earlier colonists of Virginia to inhabit that country, either in a wild or in a cultivated state.* Smith's testimony.

Under the head, or date, of 1613, Captain Smith says, that by the return of the ship Elizabeth to Virginia, from England, potatoes were brought into the country. "In this ship were brought the first potato roots, which flourished exceedingly for a time, till by negligence they were almost lost (all but two cast-away roots) that so wonderfully have increased, they are a maine releefe to all the inhabitants." On the margin of the page, we read "A strange increase of potatoes*." His account of the introduction of the potato into Virginia.

This

* The Generall Historie of Virginia, New England, and the Summer Isles: with the names of the Adventurers, Planters, and Governours, from their first beginning An: 1584, to this present 1624, &c. By Captaine John Smith, sometymes Governour in those Countryes, and Admirall of New England. Page 179. London: 1624.—It may not be amiss to take notice, in this place, of some of the roots which Captain Smith mentions as *indigenous* in Virginia. "The chiefe root they (the Indians) have for food is called *tockawhoughe*. It groweth like a flagge in maarishes. In one day a salvage will gather sufficient for a weeke. These roots are much of the greatnesse and taste of *potatoes*. They vse to cover a great many of them with oke leaues and ferne, Roots mentioned by Smith as indigenous to Virginia.

This an important fact.

This is, certainly, as I have said, an important passage in the history of the solanum tuberosum, and indeed in the history of the diffusion of esculent vegetables over the world. The potato has most confidently been supposed to be a native of Virginia. From this portion of North America sir Joseph Banks imagines it was brought into Britain, on or about the 27th of July, 1586. Another writer supposes that it was brought from Virginia into Ireland in the year 1623.

Different dates assigned to the importation of the potato.

Not a native of Virginia,

It is now, I think, most satisfactorily shown, that the potato is not one of the indigenous plants of Virginia; and, of course, that it could not have been brought from that country, as early as the year 1586. It is shown, that, so far from being a native of Virginia, this country received its *first* potatoes from Britain, into which they must have been introduced from some other and more southern part of America, by the ship Elizabeth, in the year 1613, about ten years before the period at which these roots are supposed by Mr. Willdenow to have been brought into Ireland from America.

but carried thither from Europe

10 years before some suppose it to have been brought to Ireland from America.

We see too, that, after flourishing very well for a time, the crops were, in a great measure, lost; and that the stock of a very important root was happily preserved by "two cast-away roots," and became, in the course of a very few years, "a maine releefe to all the inhabitants" of a country, the *openawk*, *tockawhoughe*, and similar plants of which, are of small value in comparison of the solanum tuberosum of South America.

ferne, and then cover all with earth in the manner of a cole pit; over it, on each side, they continue a great fire 24 houres before they dare eat it. Raw it is no better then poyson, and being rosted, except it be tender and the heat abated, or sliced and dried in the sunne, mixed with sorrell and meale or such like, it will prickle and torment the throat extreamely, and yet in summer they vse this ordinarily for bread." *Historie*, &c. pages 26, 27. This is certainly not solanum tuberosum. I take it to be a species of arum, and I think arum virginicum. "They have another roote which they call *wighsacan*." But this Smith mentions as a medicine. He also mentions *pocones* and *musquaspen*. The first of these is used both as a pigment and medicine: the latter merely as a pigment. *Not a word about any thing like solanum tuberosum.*

Sir

Sir Joseph Banks is not the only late writer, whose correct acquaintance with subjects of natural history entitle their opinions and conjectures to attention, that has assumed it as a fact, that the solanum tuberosum was originally found in Virginia. Sir J. Banks not singular in his opinion of the native country of the potato.

The learned Mr. Willdenow, a botanist of the first order, says—"After America was discovered, many plants were imported, and grew in our climate. The potato was first described by Caspar Bauhin in 1590; and sir Walter Raleigh, in the year 1623, distributed the first which he brought from Virginia, in Ireland, whence all Europe got them*". Willdenow.

This passage contains some errours, which it may not be amiss to correct. Mistakes in this passage,

I. Sir Walter never was in Virginia, though several authors, beside Mr. Willdenow, seem to suppose, that the illustrious Englishman visited, in person, this portion of America. respecting sir Walter Raleigh.

II. In 1623 Raleigh was not living. Five years before this period (in October 1618), he lost his head upon the scaffold, to the eternal disgrace, if not of his nation, at least of the feeble monarch, who then presided over it.

III. There is, I think, no proof whatever, that Ireland was so exclusively the first European depot of the potato, as Mr. Willdenow supposes it to have been.

Mr. Loskiel remarks, "Potatoes are originally a North American root, and are said to have been first brought to Europe by sir Walter Raleigh. They are cultivated by some of the Indians†". Loskiel.

On the subject of the potato, Baron de Humboldt has said a great deal; and it is evident, that he considers the history of this root as intimately connected with the history of the Americans. Von Humboldt.

"The potato," observes my ingenious friend (with whom I have passed many hours in useful conversation), "pre- The potato

* The Principles of Botany, and of Vegetable Philosophy. British translation, p. 390, Edinburgh, 1805.

† History of the Mission of tde United Brethren among the Indians in North America. By George Henry Loskiel. Part I, p. 69. London, 1794.

sents

not known in Mexico before the arrival of the Spaniards.

sents us with another very curious problem, when we consider it in an historical point of view. It appears certain, that this plant was not known in Mexico before the arrival of the Spaniards. It was cultivated at this epocha in Chili, Peru, Quito, in the kingdom of New Granada, in all the Cordillera of the Andes, from the 40° of south latitude to the 50° of north latitude. It is supposed by botanists, that it grows spontaneously in the mountainous part of Peru. On the other hand, the learned, who have inquired into the introduction of potatoes into Europe, affirm, that the potato was found in Virginia by the first settlers sent there by sir Walter Raleigh, in 1584. Now how can we conceive, that a plant, said to belong originally to the southern hemisphere, was found under cultivation at the foot of the Alleghaney mountains*; while it was unknown in Mexico, and the mountainous and temperate regions of the West Indies? Is it probable, that Peruvian tribes may have penetrated northward to the banks of the Rappahannoc†, in Virginia? or have potatoes first come from north to south, like the nations, who, from the seventh century, have successively appeared on the table-land of Anahuac? In either of these hypotheses, how came the cultivation not to be introduced or preserved in Mexico?"

* Why at the foot of the Alleghaney mountains? admitting, that the potato was really brought from Virginia in 1585 or 1586, it did not come from the *foot* of these North American Andes. No Englishman had penetrated, at this early period, as far as the Alleghaney chain; or even, I believe, as far as the more eastern chains, called the Blue Ridge and North Mountain. The Spaniards, indeed, near half a century earlier than this period, had even *crossed* these mountains in a more southern clime. I allude to the march of F. de Soto's army. And these Spaniards, I may here add, found *no potatoes*. Mr. Humboldt takes the openawk to be the potato. But the openawk was not a mountain plant.

† Why mention the Rappahannoc? Has any one said, that the openawk was found *especially* in the neighbourhood of this river? I have no doubt, however, that Peruvian tribes, that is, Indians specifically, and even varietally, the same, inhabited both the valley of Quito, and the lands which border upon the Rappahannoc, in Virginia; and even upon much more northern streams. But the discussion of this subject belongs to another essay.

"We

"We know not a single fact," continues Mr. Humboldt, "by which the history of South America is connected with that of North America. In New Spain, the flux of nations was from north to south. A great analogy of manners and civilization has been thought to be perceived between the Toultecas, driven by a pestilence from the table-land of Anahuac in the middle of the twelfth century, and the Peruvians under the government of Manco Capac. It might, no doubt, have happened, that people from Aztlas advanced beyond the isthmus or gulf of Panama; but it is very improbable*, that by migrations from south to north the productions of Peru, Quito, and New Granada, ever passed to Mexico and Canada". No single fact known to connect the history of North and South America.

"From all these considerations, it follows," says Mr. Humboldt, "that, if the colonists sent out by Raleigh really found potatoes among the Indians of Virginia, we can hardly refuse our assent to the idea, that this plant was originally wild in some country of the northern hemisphere, as it was in Chili. The interesting researches carried on by Messrs. Becmaney, Banks, and Dryander, *prove*, that vessels, which returned from the bay of Albermarle in 1586, first carried potatoes into Ireland; and that Thomas Harriot, more celebrated as a mathematician than as a navigator, described this nutritive root by the name of *openawk*. Gerard in his *Herbal*, published in 1597, calls it Virginian potatale, or *novembega*. The very name by which Harriot describes the potato, seems to prove its Virginian origin. Were the savages to have a word for a foreign plant, and would not Harriot have known the name *papa*† ?" The openawk of Virginia supposed by Humboldt to be the potato.

Baron Humboldt seems, upon the whole, to think the solanum tuberosum was really found in Virginia; and that it is the openawk of Herriot. He intimates too, that it was found in a *cultivated* state, in that country. For this suspicion there is no authority. Even the openawk was not cultivated. It is evident, however, that Mr. Humboldt, Another mistake of Humboldt,

* I think it very probable.

† Political Essay on the kingdom of New Spain. By Alexander de Humboldt. Vol. II, p. 344—351. English translation. New York, 1811, octavo.

while

while carrying on his speculations concerning the native country of the potato, lies under the pressure of a favourite theory,—in my opinion a very feeble one,—"that there is not a single fact, by which the history of South America is connected with that of North America". I shall examine this theory in another place. I shall even endeavour to show, by an attention to different species of vegetables, *which were unquestionably found in a cultivated state in the two Americas*, that there is not only a "single" fact, but that there are *many* facts, by which the history of South America is connected with that of North America.

who is prejudiced by a favorite hypothesis.

The history of North and South America connected by various facts.

BENJAMIN SMITH BARTON.

Philadelphia,
January the 13*th*, 1812.

XI.

On the Production of Electrical Excitement by Friction. *By* J. D. Maycock, *M. D.*

To W. NICHOLSON, Esq.

SIR,

Law of electrical excitement.

THE interesting discoveries of Wilcke, Æpinus, Volta, and other experimenters on the two electricities, together with the result of several experiments by Dr. Davy*, and the facts, which I lately communicated to you†, lead, by a fair induction, to a general law, which may be thus expressed: *The contact and separation of dissimilar bodies operate as a cause of electrical excitement*; *and the charge, which is assumed, after separation, by one body, is precisely opposite to that, which is acquired by the other.*

It is general.

The existence of this law, in relation to a variety of bodies, is fully demonstrated; and, as far as the investigation has proceeded, it appears to affect all in a greater or less degree: I think, therefore, we are warranted, by every princi-

* Philos. Trans. 1807. † Journ. vol. XXIX.

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ple of rational theory, to receive it as a general and well established law in electrical science. I have already endeavoured to show, that the decomposition of bodies by galvanism depends on the operation of this law; and that the commonly received opinions, respecting the excitement of the galvanic battery, are entirely inconsistent with it. The object of the following pages is to prove, that the excitement of bodies by friction is referrible to the same general law, and is the effect of the contact and separation of dissimilar bodies.

operates in galvanic decomposition, and is the cause of excitement by friction.

In what manner the contact and separation of dissimilar bodies operate as a cause of electrical excitement, I do not pretend to explain; nor am I, at present, anxious to determine, how much of the effect is to be attributed to the contact, how much to the separation: it is sufficient, for the purpose of my argument, to repeat, that no excitement is visible as long as the bodies are in contact; and that, immediately as they are separated, they indicate opposite electricities.

How it causes it not explained.

It must be obvious, that, while we are drawing one body over another, a number of points in the surface of the rubber are first brought into contact with a corresponding set of points in the surface of the body rubbed; that they are then separated from them, and brought into contact with another set of points, and so on, until the one body has passed entirely over the other. Now, at each separation, if the bodies be of different kinds, whether conductors or nonconductors, the general law, we have stated, must operate, and opposite electrical states must be excited in the separated particles. So far, therefore, the excitement by friction, and the excitement by contact and separation, appear to be referrible, in a general manner, to the same principle. We shall now proceed to a more particular consideration of the subject.

Contact and separation the necessary effects of rubbing.

The principal facts, relative to the excitement of bodies by friction, may be expressed in the five following propositions. 1. *To produce excitement by friction, it is essentially necessary, that one or both the bodies employed in the operation be of the class of electrics.* 2. *If two electrics, or an electric and an insulated conductor be employed, the one body will, after the operation, indicate an electricity opposite to* *that*

Principal facts relative to excitement by friction.

that which is indicated by the other. 3. *The effect of friction performed with one combination of dissimilar bodies is different from that which is produced by any other combination.* 4. *The friction of two bodies, similar in all respects to one another, produces no excitement.* 5. *If the rubber of an electrical machine be insulated, only a very slight charge can be accumulated in the prime conductor; and, under such circumstances, the action of the machine soon ceases altogether.* The agreement of the second, third, and fourth propositions with the general law is too obvious to require being pointed out: and it will not be difficult to show a perfect agreement of the first and last propositions with the same general law, and in this manner to justify its application to the excitement of bodies by friction.

These agree with the general law.

Why one of the bodies must be an electric.

In the first place, let us consider, whence proceeds the necessity of one of the bodies, employed to produce excitement by friction, being an electric. If one conductor be rubbed on another, no evident excitement is produced; for in consequence of the free communication between all parts of a conductor, and between conductors in contact, the charge is removed from each set of particles, immediately as it is excited in them; or, in other words, during the friction of such bodies on one another, opposing powers operate; the contact and separation of dissimilar bodies, tending to produce excitement; and the conducting quality, tending to destroy excitement. If either of the bodies be connected with the Earth, the electrical state of both must be precisely the same as that of the Earth: if they be both insulated, they must possess similar electrical states, as long as they are in contact by the smallest physical points; and consequently there can be no excitement in either; for when excitement is produced by the contact and separation of two insulated dissimilar bodies, one body assumes an electrical state precisely opposite to that, which is acquired by the other. When, therefore, the one conductor has been drawn completely over the other, no more excitement can remain, than what is effected by the separation of the last particles, which had been in contact; a degree inconceivably small, when shared with the whole of the bodies, to which the excited particles belong. As a confirmation of this reasoning,

I repeat, that no excitement of the Voltaic plates takes place except the extensive surface of one plate be separated, perpendicularly, from the corresponding surface of the other, so that all points of contact between the two plates be broken at the same instant. If one plate be made to slide over the other, or if, after their separation, they be connected by the smallest points, no excitement is indicated by either.

Excitement of the common electrical machine.

The result is, however, different, if the experiment be performed with two electrics, or with an electric and a conductor; for the charge, which is excited in one set of particles of an electric, is retained by them, and is not shared with the rest of the body, to which they belong; consequently, when any body is drawn over the surface of an electric, it leaves a permanent charge on all parts, with which it comes into contact. As, therefore, the particles of the cylinder or plate of a common electrical machine are separated from the rubber, they acquire a charge, which, as often as they pass near the prime conductor, is partly removed; and from the alternation of these operations, during the revolutions of the electric, a charge is accumulated in the prime conductor, until the whole of this body, if it be insulated, has acquired a degree of excitement, equal to that, which the action of the rubber is capable of giving to each particle of the electric. The prime conductor draws its charge, by degrees, from the excited particles of the plate, or cylinder, as they successively pass near it; but, from its conducting quality, gives it, at once, to any other conductor. It is evident, that no accumulation can take place in the prime conductor, while it is connected with the Earth; and it is equally obvious, that a conducting body, placed near to the prime conductor, will, by removing small charges, as fast as they are produced, effectually prevent its acquiring a high degree of excitement. We may also observe, that the action of an electrical machine becomes more energetic, the longer it is continued; for a repetition of the operations, we have explained, causes a considerable augmentation of temperature, which is favourable to the electrical action of bodies, and adapts the rubber perfectly to the electric, in consequence of which a greater number of points are excited

in a given time. The fact, therefore, which is expressed in the first proposition, and which is fully established in practice, is in all respects consistent with the proposed theory.

Necessity of the rubber's communicating with the Earth.

Now, from what has been said, it follows, that, if the rubber do not freely communicate with the Earth, it must become negatively electrified, by the same operation, that gives a positive charge to the prime conductor; and consequently less and less qualified, as the experiment proceeds, to produce a positive charge on the plate or cylinder. We therefore, perceive a sufficient reason, why, if the rubber be insulated, the prime conductor acquires only a low excitement, and the action of the machine ceases altogether in a very short time.

All the phenomena explicable on this principle.

The reasonings I have employed apply particularly to the electrical machine in its present improved state. The principles of my argument may, however, be extended to every experiment, in which excitement is produced by friction, and will, if I have not entirely deceived myself, afford a perfect and satisfactory explanation of the phenomena. I would, therefore, draw my observations on this subject to a conclusion, by stating, that the contact and separation of dissimilar bodies, which have been demonstrated to be a cause of electrical excitement, must operate whenever we employ friction, and that it is capable of producing the principal phenomena, which are excited by friction. This appears to me to form as strong a degree of evidence in favour of a doctrine, as philosophy need require.

A step towards the generalization of electrical phenomena.

The facts, sir, to which I have called your attention, do not immediately point to any bold and extensive views of nature; but they enable us to proceed one step towards a perfect generalization of electrical phenomena; and it is impossible for us to say, to what interesting truths they may ultimately lead. It will, no doubt, be admitted by every one, that an important advantage will have been gained, when we are able to reduce all the means of exciting electricity to one head; as we shall then be better qualified than we are at present, to investigate the relations, which unquestionably prevail, between the first principles of heat, light, magnetism, and electricity.

Excitement of

The excitement of the galvanic battery is a subject yet involved

volved in the deepest obscurity. All the opinions, which have been proposed to account for it, are unavoidably hypothetical, and, indeed, very unsatisfactory: every fact, therefore, which relates to it, deserves attention, although its application may not be clearly perceived. I was induced, some time ago, to try the two following experiments. I filled one of the new porcelain troughs with an acid fluid, so that the metallic plates, and their connecting arc, were completely covered. In this state, a trough of ten pair of plates, 3 inches square, decomposed water very rapidly. Anxious to know how far the division of the trough into cells is at all requisite, I placed the metals, connected by the bar, in a trough without partitions, and filled with the same kind of fluid, but no action ensued. The action which took place in the first experiment appears to be inconsistent with all our theories; and it seems not a little curious, since a communication between the cells is not an impediment to action, that no action was evinced in the second experiment.

the galvanic battery.

Two experiments with it.

It will afford me much pleasure, should these observations call the attention of your readers to the theory of electrical excitement. I trust, that, while we are successfully employing the powers of electricity in chemical analysis, we shall not altogether neglect to investigate the means by which these powers are called forth, and the laws by which their action is regulated. It has, with much injustice, been objected to theoretical pursuits, that they lead to none of the practical advantages, which interest the happiness of society. The remark is indeed true, if applied to particular discoveries; but these are to be considered only as the elements, from which physical science first took its origin, and by which it is daily nourished and supported. Let it never be forgotten, that our most perfect instruments, those which promote no less our comfort than they tend to advance our intellectual improvement, are the invaluable fruits of philophy.

Theoretical pursuits not unimportant.

I am, sir, very respectfully,

Your obedient and obliged servant,

J. D. MAYCOCK.

BATH,
March the 5th, 1812.

XII.

XII.

On the Nature of Oximuriatic and Muriatic Acid Gas, in Reply to Mr. MURRAY. *In a Letter from* JOHN DAVY, *Esq.*

To W. NICHOLSON, Esq.

SIR,

Two papers by Mr. Murray on oximuriatic gas.

SINCE I last had the honour of addressing you, two papers of Mr. Murray, in opposition to the theory of my brother, Mr. Davy, respecting oximuriatic gas, have appeared in your Journal.—I did not immediately reply to the first, in which I was more particularly concerned, because nothing in that paper required very serious attention: it contained no new facts or arguments in support of the old hypothesis, it consisted merely of observations on a former communication of mine concerning a new gas.—For this reason, and moreover because Mr. Murray promised, that an account should shortly appear of an experimental investigation he had been engaged in, I have hitherto patiently refrained.—The promised communication is now made, and it is now my intention to answer both his papers at the same time.

Why the first was not answered.

Answer to both.

To the first.

I shall be brief in my remarks on Mr. Murray's former paper. To his incorrect statements I shall oppose merely the results of my experiments. His criticisms on me, I shall, in a great measure, leave to the judgment of the public.

Grounds of the controversy.

That the reader may form some idea of the present state of the controversy, I shall quickly run over its grounds, principally directing the attention to facts.

Mr. Murray's experiment.

Mr. Murray having exposed a mixture of carbonic oxide, hidrogen, and oximuriatic gas, to light; and having found, that no carbonic oxide remained, after the addition of ammoniacal gas, and that the ammoniacal salt formed effervesced with nitric acid; concluded, that the salt was a mixture of carbonate and muriate of ammonia—that the oximuriatic gas had been decomposed, and consequently that Mr. Davy's theory, in which it was considered as a simple substance, was erroneous.

Repeating

Repeating this experiment, I obtained a similar result: but, as the decomposition of the salt with effervescence was occasioned by *nitric acid*, I did not hastily draw the conclusion, that carbonic acid gas was directly formed without the intervention of water.

Repeated with the same result.

Prosecuting the inquiry I ascertained the existence of an acid gas, consisting of oximuriatic gas and carbonic oxide, which combined with ammonia, and formed a neutral salt, that was not decomposed by *acetic acid*, but with effervescence by *nitric acid*; and which, in all its characters, was as essentially different from a mixture of carbonate and muriate of ammonia, as the new gas itself was from a mixture of the carbonic and muriatic acid gasses. Hence I inferred, that the effervescence Mr. Murray observed was owing to the decomposition of the new ammoniacal salt, formed, I conceived, in his experiment; and that he would have observed no effervescence, had he used the acetic acid instead of the nitric.

A new acid gas, which occasioned the effervescence.

But Mr. Murray was not satisfied with this explanation. He still continued to assert, that the production of carbonic acid in his experiment "was established beyond the possibility of doubt."

The explanation not satisfactory to Mr. Murray.

I grant, that the effervescence is owing to the disengagement of carbonic acid gas. But I deny, that the carbonic acid gas had previously existed in the ammoniacal salt. If this salt was a mixture of carbonate and muriate of ammonia, it would have effervesced with the acetic, as well as with the nitric acid. And I maintain, that the results of my experiment did in no way warrant the liberty, which Mr. Murray has taken with them, of asserting, that they confirmed his statement respecting the direct formation of carbonic acid gas.

The carbonic acid evolved did not previously exist in the salt.

I shall silently pass over the general reasoning advanced by Mr. Murray, in favour of the conclusion he drew from his experiments on the mutual action of the three gasses. I have only to observe, that I have made the experiment, and have given an account of it in a paper sent to the Royal Society on the new gas, and that the result of it was a mixture of the new gas and of muriatic gas. I repeat, that Mr. Murray would not have inferred the formation of carbonic acid

Mr. Murray would not have drawn the inference he did, had he used a different acid.

acid gas, had he used an acid, which did not decompose the new ammoniacal salt.

Answer to Mr. Murray's objection of an inconsistency respecting the new gas;

Mr. Murray has attempted to point out an inconsistency in my account of the new gas. He conceives, that it does not decompose water; and consequently, that its ammoniacal salt cannot, when acted on by an acid. This inconsistency is merely imaginary. The fact is, that the gas, immediately on coming into contact with the water, is decomposed, and converted into the same gasses, that the ammoniacal compound yields when acted on by nitric acid; viz. the carbonic and muriatic. In my first notice of the gas I mentioned its being apparently slightly absorbed by water, only among its most obvious qualities, those which made the first impression on me, and led me to consider it as a new substance.

to his assertion, that he has shown Dr. Davy's opinion to be hypothetical;

As the facts accumulate in opposition to the old hypothesis, Mr. Murray's faith in it seems proportionably to strengthen. He speaks with great confidence of what he conceives he has done. He says, "Mr. Davy's opinion, which was first held out as a genuine theory, admitting of no doubt as being a simple expression of facts, has been shown to be a hypothetical explanation of phenomena. And as an hypothesis not a single proof has been given of its truth." Could assertion supply the place of argument, Mr. Murray certainly would carry his point, and effect all that he conceives he has already done. How he has shown Mr. Davy's theory to be an hypothesis, I confess myself totally at a loss to understand. He has advanced no arguments, that have not been answered; no experiments, the accuracy of which has been admitted; and most of his after papers contain little more than what appeared in his first. What I considered Mr. Davy's theory I still continue to consider it. If it is not an expression of facts, in all its essential parts, to the exclusion of hypothesis, I am greatly mistaken.

and to his remarks on Mr. Davy's style.

Mr. Murray indirectly charges me with a want of candour, calmness, and forbearance, at the commencement of the controversy. Let others decide, whether I deserve this charge, and whether Mr. Murray himself does not, in some measure, merit it. I acknowledge, that I attacked, in my first paper, the old hypothesis with a little warmth, though

I

I trust without any arrogance. I did so, because I was perfectly satisfied of the truth of the theory I ventured to defend; and because I opposed opinion merely, and not authorities and persons.—And I hope, if I have been guilty of any impropriety of style, this will in some degree extenuate the fault.

Answer to the last paper.

I shall now proceed, briefly to consider the other paper.

Principles

It is a Baconian principle, not to admit the existence of imaginary things. And it is a principle of modern chemistry, that all bodies, that have not yet been decompounded, are to be considered as simple substances. To introduce unknown bodies into chemistry is as bad, as to adopt occult causes in philosophy. Yet such a licence has been used in respect to muriatic and oximuriatic gas. The former, it has been asserted, is a compound of an unknown something and water; and the latter, a compound of the same unknown basis and oxigen. The presence of water in the one, and of oxigen in the other, instead of being proved, has been taken for granted. Mr. Murray, in his preceding papers, to remove this objection to the old hypothesis, has endeavoured to prove, that oximuriatic gas really contains oxigen; but, since all his experiments for the purpose were found to be incorrect, his attempt was not successful. In his last communication, with the same object in view, he has endeavoured to demonstrate the presence of water in muriatic acid gas, and to obtain water from it by means of a substance not known to contain oxigen.

violated with respect to muriatic and oximuriatic gas.

Mr. Murray's attempts to remove this objection.

His experimentum crucis.

As ammoniacal gas is a substance of this kind, Mr. Murray chose it, as he states, for the subject of an experimentum crucis. He added about 32 cubic inches of alkaline gas to 30 cubic inches of muriatic acid gas over dry mercury. The salt formed was collected in the open air, and introduced into a retort. It had the appearance of being slightly moist; and, when heated, it afforded about 1·3 grain of water: and again transferred to another vessel, and passed in the state of vapour through a heated tube containing charcoal, it yielded more water.

Such is the result of the experimentum crucis, from which

which it is most confidently concluded, that muriatic acid gas contains water, and that Mr. Davy's theory is unfounded, and not to be maintained except by means of the most unreasonable assumptions.

Its result incorrect.

At first view the result appears improbable, and opposed by several facts; and in a very short time I was convinced by experiments, that it was incorrect. The results, that led me to this conclusion, I shall describe, after I have stated more conclusive evidence.

Dr Davy repeated the experiment without obtaining water.

The muriate of ammonia, on which Mr. Murray operated, was exposed to the atmosphere in both stages of his experiment previous to distillation. Mr. Davy, my brother, particularly pointed out this circumstance to me; and at the same time informed me, that he had not observed the slightest traces of moisture in making the experiment on a large scale in exhausted vessels; and assured me, that I should not, was not the salt exposed to the atmosphere.

The experiment repeated by Mr. Davy,

In repeating the experiment, which, if accurately made, could not fail of being decisive, I used two mercurial troughs; one for preparing the gasses, the other for combining them in. About 30 cubic inches of each gas were employed. The combination made in a small retort, the capacity of which was about 3 cubic inches, and over well dried mercury; and only one cubic inch of ammoniacal gas was added at a time to one cubic inch of muriatic acid gas, so that all the muriate was collected in the upper and curved part of the retort. Heat almost sufficient to occasion the sublimation of the salt was applied for about ten minutes, but no water was produced: agreeably to my brother's result, not even the slightest traces appeared.

and no water produced:

I next followed Mr. Murray's example, and collected the salt in the atmosphere, and introduced it into another retort; when, heat being applied, water in no inconsiderable quantity was evolved as he described.

but much water appeared when the salt was passed through the air.

Thus we have a demonstration, that the water liberated in Mr. Murray's experiment was not derived from the muriatic acid gas, but from the atmosphere.

Consequently it was derived from the atmosphere.

His errour appears to have arisen partly from too great confidence placed in the accuracy of his experiment; and partly

Sources of Mr. Murray's mistake.

partly from overlooking, that a light powdery substance like muriate of ammonia, independent of its chemical attraction, absorbs water hygrometrically. Mr. Davy has informed me, that this is the case, and that muriate of ammonia so made absorbs so much, that it even deliquesces.

Muriate of ammonia so made deliquescent.

Mr. Murray's confidence in his result, which is opposed by several facts relative to muriate of ammonia, is to me more surprising than the result itself.

It is well known, that muriatic acid gas condenses its own volume of ammoniacal gas to form muriate of ammonia, which, from trials I have made of its properties, does not appear to differ in any respect from common sal ammoniac. This being the case, if water is liberated on the union of the two gasses, it should, were Mr. Murray's experiment correct, be indicated by an absorption of muriatic acid gas, provided an excess was used. I have made the experiment, but have not observed the slightest diminution of the gas added in excess.

No water to absorb muriatic gas, if added in excess.

These facts, though mentioned last, first convinced me of the inaccuracy of Mr. Murray's experiment, for they were first ascertained. They confirm the other decisive evidences already brought forward; and, if farther proof was required, I could advance additional circumstances to show, that water is not produced, when the union of muriatic acid gas and ammoniacal gas takes place. As this appears to me to be demonstrated, the necessary consequence is abiding by the experimentum crucis, and renouncing that hypothesis, to which it stands opposed: indeed Mr. Murray allows, that, should the event turn out as it has, such a step must be taken: he allows, if water is not produced, "that it may be concluded, that the water obtained in other combinations of muriatic acid gas has not preexisted in it, but is ready formed;" that Mr. Davy's theory, in short, is correct, and the old doctrine erroneous. Should he not make this acknowledgment, I think he will no longer assert, guided by his own experiment, that Mr. Davy's theory is unfounded, and that it can be maintained only by the most gratuitous assumption; or that to admit it, it is necessary to suppose

Water not produced on the union of ammoniacal and muriatic acid gas.

Necessary consequence of this decisive experiment.

suppose water to exist in ammonia, or to adopt "the hypothesis of unknown quantities of water in gasses."

With great respect, I am, sir,

Your humble servant,

JOHN DAVY.

Edinburgh, Feb. 25, 1812.

XIII.

On the Compensation Pendulums of Lieut. KATER *and Mr.* REID. *In a Letter from a Correspondent.*

To W. NICHOLSON, Esq.

SIR,

Compensation pendulum.

IN your last number I observed the description of a compensation pendulum by Mr. Adam Reid. But this pendulum I conceive to be precisely the same in principle with that invented by Lieutenant Henry Kater, and described in vol. XX, p. 214, of your Journal. The only difference appears to be, that Mr. Reid has used a rod of steel instead of wood, and that his pendulum has no means of adjusting the compensation. It is far from my intention to infer, that Mr. Reid borrowed the idea, but I trouble you with these remarks in justice to the original inventor.

I am, Sir,

Yours with much esteem,

A CORRESPONDENT.

XIV.

A short Account of a new Apple, called the Downton Pippin, in a Letter from THOMAS ANDREW KNIGHT, *Esq. F. R. S. &c. to the Secretary**.

DEAR SIR,

New variety of the pippin.

I Sent last autumn a couple of dozens of a new apple, the Downton pippin, for the inspection of the Horticultural Society,

* Trans. Hort. Soc., vol. I, p. 251.

Society, and I hope it will be thought no very humble imitation of the golden pipin, its male parent; being formed by introducing the pollen of this variety into the blossom of an apple provincially known under many names, but most generally by that of the orange pippin, which name however is by no means properly appropriated to it, for the fruit is thickly streaked with red.

How produced. The trees of both varieties were trained to a south wall, and the blossoms of the orange pippin were of course properly prepared for the experiment. The Downton pippin is, in the opinion of a committee of the Herefordshire Agricultural Society, an excellent cider apple, and the hydrometer, as well as the palate, indicates, that its expressed juice holds in solution a large quantity of saccharine matter. An excellent cider apple.

Its good qualities. The trees of this new variety grow very rapidly, and are so exuberantly* productive, that I am confident the fruit of them may be brought to market at any given price, with more advantage to the grower, than any other good apple cultivated. It ripens a little earlier than the golden pippin, but may be preserved in considerable perfection till March, if not gathered too ripe.

Other new apples. The specimens sent to the Horticultural Society grew in a cold soil, and northern exposure, nor did they afford by any means a favourable sample of this apple†. I hope next autumn to lay before them several other new varities of the apple, obtained by similar means, and which will prove well calculated to supply the place of those, which have been long cultivated, and in which the vital principle is nearly exhausted.

I remain yours,

Downton, Feb. 17, 1809. T. A. KNIGHT.

SCIENTIFIC NEWS.

Wernerian Society.

Porphyry. AT the meeting of this society on the 18th of January, prof. Jameson read a paper on porphyry, in which he de-

* Some grafts of the Downton pippin sent to the Botanic garden at Brompton in the spring of 1807, I am informed, have already produced fruit abundantly.

† (See Journal, vol. XVIII, pp. 193, 194.)

scribed

scribed several species of transition-porphyry as occurring along with gray-wacke, &c., in different parts of Scotland. He also gave a particular account of a floetz-porphyry, which likewise occurs in Scotland, and appears to belong to the old red sandstone formation. The professor conjectured, that this floetz-porphyry may be the mother-stone of the porphyritic felspar lavas, which are found in some countries: and consequently that lavas may occur in rocks of an older date, than those of the newest floetz-trap series.

Floetz-porphyry.

Lavas in rocks of older date than the newest floetz-trap.

Shark genus.

At the same meeting Mr. W. E. Leach read a description of two species of shark found in the Scottish seas, illustrative of a proposed subdivision of the genus squalus of Linnæus.

Geology of the Campsie hills.

At the meeting on the 1st of February a communication from Lieut. Col. Imrie was read, containing an account of the district of country in Sterlingshire called the Campsie Hills, illustrated by some interesting geological facts observed by the Colonel on the coast of the Mediterranean. The Campsie Hills consist of trap rocks of great thickness; under which sandstone occurs; and below this lie beds of limestone, with slate-clay, clay iron-stone, and some seams of coal. The trap is in some places distinctly columnar; and in many other places, it shows a tendency to this form. He observed, that these circumstances might give occasion to some geologists to class the trap of the Campsie district with volcanic products, of which however he saw no symptom. He then pointed out, that nature produces these forms both in the moist and in the dry way, and gave examples of both. In the *moist* way, he said, that these forms are seen in greatest perfection in warm climates; and drew his example, in this mode, from the coast of Africa, near the site of ancient Carthage; where a small lake with a deep clay bottom had been accidentally drained by the breaking down of a part of its barrier, and where the clay deposit had split into vertical columns eighteen feet high, and from a foot and a half to three feet in diameter. The example in the *dry* way he took from the island of Felacuda, one of the most westerly of the Lipari islands. In the lavas of that island, which have taken the columnar form, he mentioned having seen obsidian and pumice, which had been in flow with the lava, and are seen combined in one of its congealed streams.

Columnar trap.

This structure produced both in the moist and dry way.

The moist exemplified from Africa:

the dry from Felacuda.

Geological

Geological Society.

March the 6th. An additional notice by A. Aikin, Esq. Sec. G. S. respecting a green waxy substance found in the alluvial soil near Stockport was read. The purport of this notice was to mention the discovery of a similar substance at the foot of the hill Menil Montant near Paris by Mr. Patrin. It there occurs in alluvial sand accompanied by fresh-water shells. Green waxy substance in alluvial soil.

A communication addressed to the Secretary by the Hon. Henry Grey Bennet, M. P., respecting a whin dike in Northumberland, was read. Whin dike in Northumberland.

The dike here described, is best seen at Beadnel bay where it forms a kind of pier about 27 feet wide and 300 yards long. It rises in a perpendicular position through several beds of stratified rocks, without occasioning any change in their dip or direction. But the qualities of the different strata, where they are in contact with the dike, differ very notably from those exhibited by the same strata at a little distance from the dike. The limestone in particular of both the beds, that are cut thrrough, is harder, more granular and sparry in the vicinity of the dike, and is, farther, incapable of being burnt into good lime. Its effects on the strata in contact with it.

The reading of Mr. Phillips's paper on the native oxide of tin of Cornwall was continued. Before entering into the crystallographical history of this substance, Mr. P. makes some remarks on the kind of crystals best adapted for goniometrical researches, and states his reason for preferring the more minute crystals to the larger ones, and the reflecting goniometer of Dr. Wollaston to that in common use. He then proceeds to state the means, by which he succeeded in obtaining fractures exhibiting the structure of the crystals, from which it appears, that their primitive form is that of an octaedron composed of two pyramids united by their bases, which are square, and that this is farther divisible through both its diagonals into irregular tetraedrons. Native oxide of tin. Minute crystals and Dr. Wollaston's goniometer preferable for ascertaining the angles.

March the 20th. The reading of Mr. Phillips's paper on the native oxide of tin of Cornwall was concluded. After describing Oxide of tin.

describing the primitive figure of this substance, Mr. P. proceeds to an enumeration and description of those modifications, with their varieties, which have been observed by him, and specimens of which are at present in his cabinet.

Twin crystals. After describing twelve modifications, the paper concludes with details of those compound crystals usually called macles; of the still more compound ones, which are formed by the junction of two macles; and of the most compound of all, which are macles of macles.

Castle hill, near Newhaven in Sussex. A description of Castle hill near Newhaven in Sussex, by Hen. Warburton, Esq., Memb. of the Geo. Soc., was read. Castle hill is a small circular elevation, composed of nearly horizontal beds, lying above the chalk in the following order, beginning from the most recent:—1, Sand and rounded flint pebbles. 2, A congeries of oyster shells. 3, A bed of broken bivalve shells, chiefly of the genus *Venus*. 4, A bed of blue clay, enclosing a seam of martial pyrites 3 or 4 inches thick, composed entirely of casts of bivalve and turbinated shells. 5, A bed of indurated marl, the lower part of which is obscurely slaty, and contains between its laminæ leaves apparently of some tree of the willow tribe converted into coal. 6, A seam of coal three or four inches thick. 7, Marl, of a sulphur yellow colour, including large crystals of gypsum. 8, Sand. 9, Chalk.

Accidental sublimation of silex. A notice respecting an accidental sublimation of silex by Dr. Mac Culloch, Mem. Geo. Soc., was read. A mixture of the oxides of tin and lead was put into an earthen crucible, and covered by another inverted over it: the mass was exposed to a high heat, and on opening the crucibles the empty part of each of them was found lined with capillary shining crystals, which by the usual methods of analysis were proved to be pure silex.

To Correspondents.

I find myself again unfortunately obliged to postpone my answer to A. H. Z. till next month.

A

JOURNAL

OF

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AND

THE ARTS.

SUPPLEMENT TO VOL. XXXI.

ARTICLE I.

Observations and Experiments on Vision. By WILLIAM CHARLES WELLS, *M. D. F. R. S.**

1. I WAS consulted, in the beginning of the year 1809, upon a disease of vision, which, as far as I know, has not hitherto been mentioned by any author. The subject of it was a gentleman about thirty-five years old, very tall, and inclining to be corpulent. About a month before I saw him, he had been attacked with a catarrh, and as this was leaving him, he was seized with a slight stupor, and a feeling of weight in his forehead. He began at the same time to see less distinctly than formerly with his right eye, and to lose the power of moving its upper lid. The pupil of the same eye was now also observed to be much dilated. In a few days the left eye became similarly affected with the right, but in a less degree. Such was the account of the case, which I received from the patient himself, and from the surgeon who attended him. The former added, that previously to his present ailment his sight had always been so good, that he had never used glasses of any kind to improve it. On examining his eyes myself, I could not discover in them any other appearance of disease, than that their pupils, the right particularly, were much too large,

Uncommon disease of vision.

* Phil. Trans. for 1811, p. 378.

and that their size was little affected by the quantity of light which passed through them. At first, I thought that their dilatation was occasioned by a defect of sensibility in the retinas; but I was quickly obliged to abandon this opinion, as the patient assured me, that his sensation of light was as strong, as it had ever been during any former period of his life. I next inquired, whether objects at different distances appeared to him equally distinct. He answered, that he saw distant objects accurately, and in proof told me what the hour was, by a remote public clock; but he added, that the letters of a book seemed to him so confused, that it was with difficulty he could make out the words which they composed. He was now desired to look at a page of a printed book through spectacles with convex glasses. He did so, and found that he could read it with ease. From these circumstances it was very plain, that this gentleman, at the same time that his pupils had become dilated, and his upper eye-lids paralytic, had acquired the sight of an old man, by losing suddenly the command of the muscles by which the eye is enabled to see near objects distinctly; it being known to those, who are conversant with the facts relating to human vision, that the eye in its relaxed state is fitted for distant objects, and that the seeing of near objects accurately is dependent upon muscular exertion.

Another instance.

The disease of which I have spoken is perhaps not extremely rare. For having related the preceding instance of it to Mr. Ware, a fellow of this society, he was kind enough shortly after to send to me a young woman, who appeared to be likewise affected with it. But as I saw her only once, and had not then sufficient time to examine her case minutely, I speak with diffidence concerning its nature.

Effect of the juice of belladonna on the eye.

II. After I had reflected frequently upon these cases, it occurred to me, that, as the juice of the herb belladonna, when applied to the eye, occasions the pupil to dilate considerably, and to become unalterable by light, an effect might at the same time be produced by it upon vision, similar to that which I have just described. I had, indeed, in the course of a few years immediately preceding, applied belladonna several times to my own eyes, without observing

serving any change in my sight, beyond what I referred to the increased size of the pupils; but as I had not looked for any other, I thought it possible, that some additional one might have happened, without my having perceived it. I resolved therefore to make the experiment anew. But to conduct it with precision, it was previously necessary to know, to what extent I possessed the faculty of adapting my eyes to different distances. On this subject I had made many experiments with great care, nearly twenty years before, and had ascertained*, that with my left eye, which was more perfect than the right, I could bring to single points on the retina pencils of rays, which flowed from every distance, greater than that of seven inches from the cornea. In the mean time, however, my eyes had altered considerably, with respect to their seing near objects distinctly, and I had, in consequence, been obliged, not only to use convex glasses, but to change them several times for others of higher power. No dependance therefore being now to be placed on my former experiments, in regard to the present state of my sight, I repeated them, and found, to my great surprise, that the power I once possessed of adapting my eyes to different distances was entirely gone; in other words, that I was now obliged to regard all objects, whether near or remote, in the same refractive state of those organs. I found also, that my eyes, considered as mere optical instruments, were nearly the same as they had been in my youth, and that the convex glasses which I used did very little more than supply, with respect to near objects, the place of a living power which I had lost, without compensating, except in a very small degree, for any alteration in the external shape of the eye, or any change in the configuration of its interior parts. I ascertained, for instance, that to give my left eye the refractive power which it formerly possessed while in its most relaxed state, that by which it was enabled to bring a pencil of parallel rays to a point on the retina, a glass of thirty-six inches focus was fully sufficient; whereas to produce an equal effect upon rays proceeding from a point at the dis-

State of the author's eyes.

Power of adapting them to distance lost.

* Essay on Single Vision with two eyes, &c. p. 137.

 tance

tance of seven inches from my eye, the other extremity of my ancient range of perfect vision, I was now obliged to employ a glass having a focus of only six inches. I regret much, that I had not made such experiments frequently before, as I think it very probable, that I should have found a period in the progress of my vision to its present state, in which my capacity of seeing distant objects was the same as in my youth, and when therefore the whole of my imperfect vision of near objects would have been owing to a loss of the muscular powers of my eye.

Experiments on an old eye wanting the crystalline inconclusive.

As there can be no good reason for supposing, that the changes which have occurred in my eyes are different from those, which the eyes of by far the greater number of persons, who are not short-sighted, undergo at the approach of old age, it is evident, that the experiments of Dr. Young * on the eye of Hanson, whom the learned author considered as a very fair subject for such trials, furnish no proof, that the want of the crystalline lens disables a person from having perfect vision at different distances: for as Hanson was sixty three years old, it is highly probable that the results of the experiments would have been exactly the same, if he had still possessed that part of his eye.

Experiments with belladonna.

III. Having discovered, that my own eyes were unfit for the experiments, which I wished to be made with belladonna, I instructed an ingenious young physician, Dr. Cutting, from the island of Barbadoes, and now residing there, in the manner elsewhere described by me †, of ascertaining his range of perfect vision by means of luminous points. This he found, in consequence, to begin, with respect to his left eye, at the distance of six inches, and not to terminate at the distance of eight feet; beyond which he could not see clearly the object, with which he had hitherto made his experiments, the image of the flame of a candle in the bulb of a small thermometer. The flame of a lamp, distant about sixty yards, gave a faint indication of its rays meeting before they fell upon the retina; the rays from a star had very evidently their focus a little before that membrane.

* Phil. Trans. 1801, p. 66: see Journal 4to series, vol. v.

† Essay on Single Vision, &c. p. 116.

brane. He now applied the juice of belladonna to his *left eye*. Half an hour after, when his pupil was but little dilated, perfect vision commenced at the distance of seven inches; in fifteen minutes more, it began at the distance of three feet and a half. When his pupil had acquired its greatest enlargement, the rays from the image of the flame of a candle, in the bulb of a small thermometer at the distance of eight feet, could not be prevented from converging to a point behind the retina. The rays from lamps still more distant, and from stars, had their focusses at the same time on the retina. This state of vision continued, in its greatest extent, to the following day; and it was not *till the ninth* day after the application of the belladonna, that he completely recovered the power of adapting his eye to near objects. While his left eye was thus affected, the vision of the right remained unaltered.

Dilatation of the pupil not always proportional to the range of vision.

Dr. Cutting remarked, while his left eye was returning to its natural condition, that the diminution of the pupil, and the increase of the range of perfect vision, did not keep regular pace with each other; but that, after his pupil had nearly returned to its former size, his capacity of adapting the eye to different distances was still very limited. As these effects therefore are not inseparably connected, they may occur in others in a different manner from that which he observed. A great degree of dilatation, for example, may take place in the pupil, without a total want of the power to adapt the eye to different distances.

The experiment repeated with the other eye.

Though I could not doubt the accuracy of Dr. Cutting's observations, more especially as the altered state of his eye had lasted a considerable time, and as he had not been prevented by other occupations from attending minutely to the appearances, which were consequent upon it; yet, as he was the first person who had ever applied belladonna to his eye, for the purpose which has been mentioned, and as the results had been remarkable, I requested him to repeat the experiment with his other eye. He complied with my desire, and found, that the appearances which followed were similar to those, which had been produced by the application of belladonna to his left eye.

It

The belladonna seemed to have done something more than suspend the adapting power.

It will, perhaps, be thought extraordinary, that Dr. Cutting's eye in its relaxed state, before the application of the belladonna, brought parallel rays to a focus anterior to the retina; but that similar rays met in a point upon the retina, while the eye was under the full influence of that substance; as it may hence seem, that the belladonna had done more than merely suspend the exercise of the power, by which the eye is fitted to see near objects distinctly. An observation drawn from the former state of my own sight will, I expect, make this matter plain.

Different appearances of stars on the eye.

When I enjoyed the faculty of adapting my eyes to objects at different distances, the rays of a star, which was viewed attentively by me, always met in a point a little before the retina*; whence I at first concluded, that my eye was unfit for accurate vision by parallel rays. But I afterward found, that if I looked at a star carelessly, its rays had then their concourse on the retina. In the former case, from long habit, originating in my having chiefly viewed near objects with attention, some small exertion was made for the accurate view of a distant object, though none was requisite; in the latter, all demand for exertion ceasing, my eye fell into the most relaxed condition, that by which it was fitted for parallel rays. Dr. Cutting's eye seems to have been similar to what my own once was, in regard to such rays; but as he had not acquired the faculty of viewing a distant object, without making some exertion, the rays from a star crossed one another in his eye before they came to the retina. The capacity, however, of making any exertion was taken away by the belladonna, and pencils of parallel rays were, in consequence, brought to points upon that membrane.

Effects of age on short sight.

IV. Being now in possession of a new instrument, I next attempted to gain, by means of it, some illustration of the changes, which the vision of short-sighted persons undergoes from age.

General mistake respecting it.

It has been very generally, if not universally, asserted by systematic writers upon vision, that the short-sighted are rendered by age fitter for seeing distant objects than

* Essay on Single Vision, &c. p. 138.

they

they were in their youth. But this opinion appears to me unfounded in fact, and to rest altogether upon a false analogy. If those who possess ordinary vision, when young, become from the flatness of the cornea, or other changes in the mere structure of the eye, long-sighted as they approach to old age, it follows, that the short-sighted must, from similar changes, become better fitted to see distant objects. Such appears to have been their reasoning. But the course pursued by nature seems very different from that which they have assigned to her. For of four short-sighted persons of my acquaintance, the ages of whom are between fifty-four and sixty years, and into the state of whose vision I have inquired particularly, two have not observed that their vision has changed since they were young, and two have lately become, in respect to distant objects, more short-sighted than they were formerly. As the manner, in which this change has occurred, is unnoticed, I believe, by any preceding author, I shall here relate the more remarkable of the two cases.

Short sight for distant objects increased.

A gentleman, who is a fellow of this society, became short-sighted in early life; and as his profession obliged him to attend very much to minute visible objects, he for many years wore spectacles with concave glasses almost constantly, by the aid of which he saw as distinctly, and at as great a variety of distances, as those who enjoy the most perfect vision. At the age of fifty, however, he began to observe, that distant objects, though viewed through his glasses, appeared indistinct, and he was hence led to fear, that his eyes were affected with some disease. But happening one day to take up, in an optician's shop, a single concave glass, and to hold it before one of his eyes, while his spectacles were on, he found to his great joy, that he had regained distinct vision of distant objects. With regard to such objects, therefore, he had lately become shorter sighted than he had formerly been. But along with this change, another occurred of a directly opposite kind. For when he wished to examine a minute object attentively, such as he used to see accurately by means of his spectacles, he now found it necessary to lay them aside, and to employ his naked eye. He had become, therefore, in respect to near

Short-sighted persons become more so for remote objects, less so for near ones.

near objects longer-sighted. The power, consequently, in this gentleman, to adapt the eye to different distances, is either totally lost or much diminished; but the point, or small space to which his perfect vision is now confined, instead of being the most remote to which he could formerly accommodate his eyes, as is commonly the case with the ordinarily sighted when they are becoming old, is now placed *between* the two extremes of his former range of accurate vision. The eyes of the other short-sighted person, a physician of considerable learning, whose vision has been altered by age, have been affected in a similar manner, but not in so great a degree.

Similar instance.

Range of perfect vision lessened both ways by age.

As the only change, which had occurred from age in the sight of such of my acquaintance as were considerably myopic, was a lessening, on both sides, of their range of perfect vision, I conceived, that this might be the ordinary procedure of nature in such cases, and that it might be imitated, in a young short-sighted person, by the application of belladonna to his eyes. I have hitherto not been able to obtain permission to make the experiment on any young person, who is very short-sighted. Two gentlemen, however, who are somewhat short-sighted, have readily submitted to it; one of them, Mr. Blundell, a diligent and ingenious student of medicine; the other, Mr Patrick, a well educated young surgeon in London. The first experiment was on Mr. Blundell, and the apparent result was, that the range of his accurate vision was considerably diminished at both ends, but not annihilated. Mr. Blundell, however, afterward informed me, that he repeated the experiment with more care in the country, and found, that in one eye the nearest point of perfect vision was moved forward about two thirds of the whole range, and in the other about one third; but that, with respect to both eyes, the most remote points of the ranges were unchanged. He added, that while one eye was under the influence of the belladonna, the other became shorter-sighted than it had been before; but the difference was not so great, as to induce me to place entire confidence in the justness of his observation. I think it right to mention here, that from mistake I applied only two thirds of the ordinary quantity of belladonna to his eye, in the

Experiments with belladonna on a short-sighted person.

the first experiment; and that he probably, in consequence of my example, applied no more when he made the second; as this might have been the reason, that during both experiments he retained, in part, the capacity of adapting his eyes to different distances.

The experiment on Mr. Patrick was conducted by myself, after he had been frequently exercised in observing the extent of his perfect vision. The results were similar to those which had been remarked by Dr. Cutting. The power of altering the adaptation of his eye, according to the distance of the objects viewed, was for some time entirely lost, and his sight became accurately fitted for such only, as were placed at the farther extremity of his former range of perfect vision. While one eye was under the influence of the belladonna, the vision of the other was unaffected.

Another experiment.

The juice affected only the eye applied to.

From these experiments it seems probable, that belladonna will in no case produce the same effect upon a young short-sighted person, that age has produced in the two instances of which I have spoken. I expect, however, to have an opportunity of repeating the experiment on two persons, who are very considerably short-sighted; and I shall take the liberty of communicating the result to the Royal Society, together with some observations I have already made, and others which I hope to make, respecting those persons, who seem to retain to extreme old age the power of seeing perfectly, as far as the accommodating power of the eye is concerned, both distant and near objects; and of others, who, after being without this power for many years, appear to regain it at a similar period of life. Probably the making known my intention may facilitate its accomplishment, by inducing other Fellows of the Society to furnish me with opportunities of increasing my knowledge of these subjects. In the mean time, I shall offer a few words upon two other topics in vision, which seem to derive illustration from my experiments with belladonna.

Did not produce the effects of age.

Farther experiments to be made.

Power of the eye retained in old age,

or recovered.

V. 1. Not only do the pupils move together, when both eyes are in a healthy state, but the pupil of one eye affected with gutta serena moves with the pupil of the other, as long as this remains sound. These facts are generally, but in my opinion erroneously, attributed to the immediate sympathy

Moving of both pupils together,

not owing to immediate sympathy.

pathy

pathy between the pupils. For when the pupil of one eye becomes dilated from the application of belladonna, the pupil of the other, so far from dilating, becomes smaller. It follows, therefore, that the size of the pupil is dependant, not only on the impression of light on the retina of its own eye, but on that also which is made on the retina of the other; and that the moving of the two together, which for the most part takes place, is only an accidental consequence of the fact which I have mentioned.

Pupil of one eye affected by the impression of light on the other.

Capacity of the eye not owing to the external muscles;

2. As the action of the external muscles of the eye has been frequently resorted to, for an explanation of its capacity to see objects perfectly at different distances, I requested Dr. Cutting to attend to this matter. He accordingly ascertained, while his eye was in its natural state, the distance from his face of the nearest point, at which he could make the two optic axes meet, this being the greatest trial of strength, to which those muscles can be exposed. Shortly after, he repeated the experiments, while, in consequence of the application of belladonna, he was without the power of adapting his eye to different distances, and found, that the strength of those muscles was not diminished. It follows, therefore, not only that the external muscles have little or no concern in fitting the eye to see distinctly at different distances, but that the same is true with respect to the cornea; as we cannot suppose, that its mechanical properties were altered by the belladonna, or at least, that it became more inflexible from the application to it of the juice of that herb. I had before made a similar experiment on myself, by comparing what had been the strength of the external muscles of my eyes twenty years ago *, with what it was after I had lost the power of altering their refractive state; but though I found no difference, yet, as their coats might have in the mean time become more rigid, I thought it right to have the experiment repeated, in a manner to which no objection could be taken.

but apparently to the crystallin

The only other part of the eye, or its appendages, which remains for enabling us to see equally well at very different distances, is the crystalline; and that it does produce this

* Essay on Single Vision, &c. p. 136.

effect,

effect, either wholly, or very nearly so, is manifest, from the necessity even young persons are under, who have lost it, of using glasses of very different convexities for near and remote objects. But in what way this important office is performed by it seems still unknown. The learned Dr. Young, indeed, as well as others before him, has supposed, that the crystalline has the power of altering its figure; but the proofs hitherto given in favour of this opinion appear very defective. In 1794, I attempted to submit its justness to the test of direct experiments, by applying to the crystallines of oxen, which had been felled from thirty seconds to a minute before, chemical and mechanical stimuli, and those of galvanism and electricity; but in no instance was any alteration of figure, or other indication of muscular power, observed. All of these stimuli were applied to the crystalline while it was surrounded by air, and some of them while it was covered with warm water. Last summer, after I knew that men lose, from increase of years, the faculty of altering the refractive state of the eye, I thought it possible, that the oxen on which I had made the experiments were too old for them. I therefore repeated most of them on the crystallines of a calf and a lamb; but still no motion was to be seen. Dr. Young has made similar experiments with a similar event; but he thinks, that no argument can hence be derived against his opinion, as neither can motion be excited in the uvea, by any artificial stimulus. In the first place, however, it is not agreeable to just reasoning, to regard an unknown thing as an exception to a general rule, rather than as an example of it; in the second, the motions of the uvea are involuntary, whereas the adaptation of the eye is, in part at least, under the command of the will; and in the third, the crystalline seems very unfit for performing the motions which he assigns to it; for if its figure be altered out of the body, by external force, it does not restore itself, but retains the shape which has been given to it, like a piece of dough, or soft clay. Possibly farther experiments with belladonna may contribute to remove the obscurity which at present surrounds this subject.

though it has not been proved, that this can alter its figure.

II. *Method*

II.

Method of producing Heat, Light, and various useful Articles, from Pit-coal. By Mr. B. Cook, *of Birmingham* *.

SIR,

Products from coal.

HAVING paid much attention to the procuring of gas, and other products, from pit-coal, I now beg leave to lay before the Society for the Encouragement of Arts &c. the results of some of my experiments on pit-coal, and the methods of producing the sundry articles, of which I have sent samples, and a japanned waiter varnished therewith. The quantity of clear tar, which may be produced from every hundred weight of coal, is about four pounds; from which a liquor, or volatile oil, may be distilled, which answers the purposes of oil of turpentine in japanning. Every gallon of tar will produce nearly two quarts of this oil by distillation, and a residuum will be left nearly, if not quite, equal to the best asphaltum. I have sent a waiter, or hand-board, japanned with varnish made from this residuum, and the volatile oil above-mentioned. This dries sooner, and will be found to answer as well as the best oil of turpentine, a circumstance which will be of immense advantage to this country; as, in the vicinity of Birmingham only, nearly ten thousand tons of pit-coal are coked or charred per week; and all the tar has hitherto been lost: but by my process, I dare venture to say, that, from the various coal works in this kingdom, more tar might be produced than would supply all our dock-yards, boat builders, and other trades, with tar and pitch, beside furnishing a substitute for all the oil of turpentine and asphaltum used in the kingdom, and improving the coke so as to make iron with less charcoal.

Japan varnish.

Quantity of valuable products.

I have sent a large specimen of the asphaltum, and three vial bottles containing as follows:—

No. 1.—A sample of the oil or spirit, being part of that which was used in making the varnish, with which the waiter sent was japanned.

* Transactions of the Society of Arts, vol. xxviii. p. 73. The silver medal was voted to Mr. Cook.

No. 2,

No. 2.—Is the same oil or spirit, a little more rectified.

No. 3.—The same, still farther rectified, and of course more clear, and freer from smell; but I find, that the specimen, No. 1, answers quite as well for varnish.

Price of the tar-spirit.

Tar-spirit is now about eight shillings per gallon, and turpentine-spirit about fifteen shillings, this latter has been, within the last two years, as high as forty-eight shillings per gallon, and the tar-spirit will answer equally as well for varnish, as you will observe by the enclosed certificate from Mr. Le Resche, on using the coal-tar-spirit, instead of the turpentine spirit.

It is preferable to oil of turpentine for varnish.

I requested Mr. Le Resche to use the tar-spirit just in the same way he would the foreign spirit, and then give the varnish to his work-people to use, without making any remark to them, which was done: he, making the varnish himself, found it mixed, and made the varnish as good in appearance as that prepared with the foreign spirit. He then gave the varnish to his work-people to use, and when they had finished their work with it, he found from their report, that it answered perfectly, and dried sooner; and when the waiter done with it was given to the polisher, it was found to polish much smoother under the hand, and take a more beautiful gloss than their former varnish, as the article now sent will show on inspection.

Coal-tar superior for ships.

I am of opinion, that the production of these articles will be of great public service. Permit me to add, that the timber of ships paid with this tar is not nearly so liable to be worm-eaten as those done with common tar.

I remain, Sir,

Your humble Servant,

B. COOK.

The following Certificate was received from Mr. Le Resche, who prepared and applied the Varnish of the Waiter sent to the Society.

Testimony of Mr. Le Resche.

THIS is to certify, that the spirit or oil, extracted from coal-tar, is every way adequate to the purpose for which it is intended, as a substitute for the foreign spirit or oil used in japanning.

Mr.

Mr. Cook having desired me to make a trial of it, the tray, or waiter, accompanying this paper, was got up in my manufactory, and is a specimen in proof of its usefulness. The varnish used for this purpose I made myself; and, instead of mixing it with the usual spirit or oil imported, which is now become excessively dear, I mixed it with the spirit, or oil, extracted from coal-tar; and I can truly affirm, that, far from its being a substitute inferior in properties to the spirit in general use, I esteem it far superior in several respects.

In the trial I made of it, I found it would dry quicker, and the varnish mixed with it would polish with more ease, bear a good lustre, and, in short, answer every requisite purpose of the foreign spirit. If to these be added the reasonable price at which it may be sold, I cannot but pronounce it a discovery, that must eventually prove greatly advantageous to the manufacturer, as well as interesting to every lover of the arts, or admirer of talent and ingenuity.

Witness my hand, the 16th day of January, 1810,

J. S. LE RESCHE, Japanner,
Church street, Birmingham.

Reference to Mr. Cook's Apparatus for preparing Gas and and other Products from Pit-Coal, Pl. IX.

Apparatus for preparing the products from pit-coal.

A, Fig. 1, Pl. IX, is a common fire-place, a stove built with brick, having cast-iron bars to put the fire in at, and a flue that goes into a chimney; A is the cast-iron pot, (which holds from twenty-five to one hundred pounds of coal, according to the size of the premises to be lighted) which hangs by the bewels or ears on a hook, suspended by a chain in this stove or furnace, about three inches above the bars of the grate, and three inches distant from the sides of the stove; the fire then flames all round this pot, and as it does not rest on the burning fuel, it is the flame only that heats it, so that it does not scale, but will last for years. The smoke &c. are carried off into a chimney. The cover *d* of the pot is made rather conical, to fit into the top of the pot close, and from the top of the cover the elbow-pipe proceeds as far as the mark *a*. The other end of the pipe with the elbow entering the water-joint is rivetted to it after; when

when the lid or cover of the pot is put on, the bewels or ears come over the elbow of the pipe that is on the lid, and a wedge is put between them and this elbow, to keep down the cover air-tight, and a little clay or loam may be luted in the joint, if any gas should escape round the cover of the pot. The other elbow B goes into a water-joint, formed of a tube affixed to the cover of the purifier C; and another tube, which passes through the lid of the purifier: the elbow-pipe then goes over the inner tube, and when put on, the jointing is made good by pouring water into the space between the tubes, which renders it air-tight. The gas, as the arrows show, passes down into the purifier C, which is rather more than half full of water; the use of this water-joint is for the convenience of removing the lid *d*, to which this pipe is attached. The purifier C is a wooden trough, with a sheet-iron top, to which the tubes are soldered, and it is fastened to the trough to keep all secure and air-tight. The sheets of iron, *e*, *f*, *g*, *h*, *i*, *k*, are alternately soldered to the iron top, and fastened to the wooden bottom. Now when the trough is half filled with water, the gas passes into it at B; and, as it can only find its way out again at R, it must pass through the water. The inner pipe B reaches under the surface of the water in the trough; now when the gas is forced into the water, it would rise to the top of the purifier, and go along in a body to the end, and out at the pipe R, if the sheets of iron, *e*, *f*, *g*, *h*, *i*, and *k*, which stand across the trough, with openings in them alternately at top and bottom, did not stop it, force it to descend down into the water, and hinder it from going any way but through these apertures, purifying it all the time it is passing through the whole body of water, until it is properly washed; it then escapes through the pipe R at the end of the trough C, passes down the pipe S, and is carried up into the reservoir or gazometer K. In the bottom of the purifier is an aperture, closed by a plug at D, to let off the ammoniacal water and tar as it is deposited, and the pipe, with the cock E at the top of the purifier, is to burn away the spare gas, when not to be used.

Purification of the gas.

There is a stop-cock placed in the main pipe at F, that when the reservoir is full, and gas is making, and cannot be

Method of burning the superfluous gas.

be used, the cock may be turned, and prevent any gas from passing from the reservoir, and by opening the cock E on the top of the purifier, and firing it, all the gas which is made more than is wanted for use may be burnt away. If this was not done, the gas would continue to find its way into the reservoir K, which would overflow and produce a disagreeable smell, which this simple way of burning it away as fast as it is made when not wanted, prevents.

Receptacle for the tar not left in the purifier.

It may in some measure happen, that, although the gas has passed through the purifier C, a small portion of tar will pass along with it, and would either clog the pipe S, or accumulate in the reservoir. To avoid this, there is placed at the bottom of the pipe S and G, before it rises into the reservoir, a jar, into which a pipe, made as shown in the drawing, conducts the tar; this collects all that passes through the purifier; it is filled with water, over which the gas passes up into the reservoir, but the tar drains down this lead pipe and deposits itself in the jar of water. The longer this pipe S is, the better, as it serves as a refrigitory.

Receptacle for the inflammable gas.

H is a plain cask, made to any proper size, and filled with water, with a cock to draw off the water when it becomes foul. The upper vessel K is made of sheet iron, rivetted together in the manner engine-boilers are made. If it is only from five hundred to one thousand gallons in size, it will require only two cross iron bars at top, and four ribs down the sides to keep it in form, with a strong ring at top; and as there is no stress on this vessel, it will ascend and descend easily without any other support or framing, the plain sheet iron sides being rivetted to the four ribs, and it is quite open at the bottom. A strong rope runs over the pullies L L, with a weight M to balance the vessel K, and assist it in rising and falling. The pipe J is that through which the gas passes from the reservoir or gazometer, and rising through the pipe T, is conveyed to all parts to be lighted.

Method of preventing the pipes from being clogged.

There is also another drain-pipe at N, for, after all the washing &c., a very small portion of tar and moisture may rise into the pipes, and perhaps in time clog them, but by laying all the pipes in the first, second, and third stories on a small descent, if any tar or moisture should rise, it will drain down all the pipes from top to bottom, and be deposited

sited in the earthen jar at N, by that means the pipes will not *clog up* in half a century. These jars must be sometimes removed and emptied, fresh water put in, as also the water in the vessel H must be changed, to keep it clean and sweet; and the water in the purifier C should be changed every two or three days: by these means the gas will be deprived of all its smell, at least as far as washing will effect it, and the apparatus will be clean.

The water to be frequently renewed.

The stop-cock at O is for the use of a master, if he wishes to lock up the gas in the reservoir, to prevent his workman &c. wasting it in his absence; as also if any pipe should leak, or a cock be out of order, in any part of the premises, by turning this cock all the gas is kept in the reservoir while the pipe is repaired, or any other alteration made; it also extinguishes all the lights when turned, if any are left burning by careless workmen, nor can they be lighted until it is opened again.

Cock for locking up the gas, and extinguishing the lights.

The whole of this apparatus is simple, and not liable to be put out of order in such a way, but that any person may put it to rights again. All the art required to make the gas is to take off the cover of the pot, and, without removing the pot, to take out the coke, and fill it with fresh coal; wedge the cover down by putting an iron wedge between the bowels or ears and the elbow of the vessel, and, if required, plaster a little clay or loam round the cover, to keep it air-tight; a fire is then to be made under it, and the whole is done. The boy, or man, who does it, must now and then look at the fire, and keep it up, until the pot is hot, and the gas is made. Now in works where lights are wanted almost always, I would recommend two fire-places, and two pots, so that when one pot is burned out, the other pot may be ready to act; for this purpose the purifier must be provided with two of the water-joints B, one communicating with each pot, and the elbow-pipe of each pot must have a stop-cock, as V. When one pot is burning, the cock in the other pipe must be stopped, that the gas may not find its way out of the purifier; and when all the gas is extracted from that pot, the cock V, leading from it, must be stopped, and the pot left to cool; while a fire is put under the other pot, its cock is opened, and a supply of gas from

Conduct of the process.

it is passed into the reservoir; by these means one of the pots is constantly supplying the reservoir with gas, and the lights are always kept burning. One purifier is all that is necessary. The cock V must be shut when either of the covers is taken up to fill the pot again with coal; and when the elbow-pipe is lifted out of the water-joint, as the cover is attached to it, a plug must be provided to fit into the water-joint pipe the moment the elbow is removed from it, or the gas will rush out of the pipe at the water-joint. But a better way would be, to lengthen the pipe of the water-joints B, and place a large cock under each of them, almost close to the top of the purifier; so that, when one pot was burnt out, by turning the cock it would keep all the gas in the purifier, while the cover was removed. No plug is necessary in this method. When people are very particular, (especially when houses or accompting-houses are to be lighted), and wish all smell to be destroyed, if they are not satisfied with washing it, and still think there is a little smell left, (and very little indeed, if any, will be left), after the washing, a small trough may be added, made in the same way as the purifier, with sheets of iron across to force the gas through the pipe R communicating with it. This trough may be filled with water, with a few lumps of lime put into it, and this water and lime changed often; on the gas being forced through this lime-water, if there was any remaining smell in it, this would completely take it away, and, as has been before observed, by changing all the waters now and then, and keeping this small trough constantly supplied with clean water and lime, the gas after passing it will ascend the pipes to the lights pure.

Farther purification of the gas.

SIR,

HAVING been from home, I was prevented from answering your obliging letter until this day.—I am much pleased, that the Society have approved of my specimens produced from pit-coal. I also feel highly gratified and honoured with their Reward. I hope to lay before you, in a short time, an account of the establishment of a work, that will be of such magnitude, as will supply this part of the country with the oil or spirit, in sufficient quantity to supersede

Intended establishment of a large work.

sede the use of turpentine &c. in japanning; and I do hope, that in time works of the same description will be established through all Staffordshire, the products of which will supply the place of a great portion of the spirit used in the kingdom, while the pitch will be of sufficient quantity to form a great part of that article now used in the dock-yards.

Requisites. All I want is support from the great coal companies and masters, to erect sufficient apparatus at the different works, to preserve the tar at all the coke furnaces, and proper means to separate the spirit from the tar. It would be a great saving to the nation, as in every hundred weight of coal coked there are lost by the present mode above four pounds of tar; and the cokes are not half so good as if they were coked in close vessels, to the exclusion of the atmospheric air. I need not describe the method by drawings of the manner of extracting the tar from pit-coal in close vessels, as that method is so generally known; it must be clear to every one, that it is procured by distilling the coal. (Present loss.)

I have, as follows, described the method I use in extracting the spirit from the tar, the process of which is so simple, that every one must understand it.

Method of extracting the spirit of tar. *Fig.* 2, Pl. IX, is a section of the furnaces, and one of the retorts, almost any number of which may work in a line, the same flue will do for all, only taking care, if any are not at work, to stop up the draught-hole, which communicates with the flue. These furnaces are built without bars, grates, or doors. A is the place where the fewel is put in to heat the retort G; the fire lies under it, and the smoke is carried off into the flue D. B is the aperture where the ashes are raked out. G is a section of the iron basin, or lower part of the retort; the dark-shaded square part shows the space the fire occupies, and the black square D the flue as it runs along the back of all the line of furnaces, and enters the chimney R, as the arrows show. I, Figs. 2 and 3, shows the upper part of the iron, earthen, or glass retort, fitted on the cast-iron basin G. K, the receiver. By this mode of setting the retorts, all the great expense of bars, doors, frames &c. is saved, and a brisker draught of air is obtained, which may be slackened at pleasure by (Apparatus described.)

covering up in part, or wholly, the fire-place A with a brick. E is a square iron plate with a circular hole in the centre, built on the top of the furnace. The cast iron basin of the retort G is made to the size of the hole in the plate: the most convenient size of the basin of the retort I find is about five or six gallons, in the shape of a deep pot, with a flanch or rim H round the edge of it; this pot or basin of the retort is put into the iron plate E, and the flanch of the retort then rests on the plate E. I is the upper part of the retort without a bottom, made to rest and fit on the flanch of the cast iron basin G. K is the receiver, larger in the mouth than the nose of the retort.

Process of extracting the spirit.

To begin the work, I fill, nearly, the iron basin of the retort G with coal-tar. I then put on the upper part of the retort I, and make it air-tight with a little sand thrown round it at the flanch H; the receiver K is put into its place, and a slow fire is put in at A, under the retort; the tar soon begins to boil slowly, or rather simmer. As soon as this begins, there rises from the tar a thick whitish vapour, which fills the glass retort; part becomes condensed, and falls in drops from the sides of the retort into the tar again, while the purer spirit rises into the neck, is condensed, and keeps dripping down the neck into the receiver; this is the spirit of the tar, and with this spirit that first arises from the tar was the waiter japanned which I sent you. The reason I chose to have the receiver wider at the mouth considerably than the nose of the retort is, that there is a strong and very volatile oily ammonia, that does not soon condense, but gets out of the receiver into the air the instant it leaves the retort, and though but in a very small quantity, so small that it is hardly possible to catch it; yet will it impregnate the air for a great distance round, with its very penetrating smell, while the spirit keeps dropping into the receiver pure and separate from the ammonia. The spirit is very volatile, quite as much so, if not more, than the spirit of turpentine, and soon evaporates if exposed to the air, which is a proof of its drying nature; indeed when used as a substitute for turpentine, it dries in the stove quite as soon or sooner, and takes equally as beautiful a polish. I sent you three specimens,

No. 1.

No. 1. is what came off the tar first. No. 2. is the same distilled a second time: and the third specimen is the second redistilled again in a glass retort; it there leaves a little pitchy residuum, and comes over clear, as the sample. Very little of the spirit is lost in passing through these different stages, if care is taken, that the fire is slow, and the process not hurried. When the spirit is perfectly extracted from the tar, there remains in the basin of the retort that beautiful pitch or asphaltum sent; which, when mixed with the spirit, forms an ingredient for making the black varnish used in japanning. If it is wished to use it as pitch, less spirit must be extracted from it. I find, that six gallons of tar will produce, if care is taken, about two gallons or two gallons and a half of spirit. A great number of retorts may be kept working by a single man; if we say only one hundred, and only worked down in a day, they will produce from two hundred to two hundred and fifty gallons of spirit, so that by increasing the number, any quantity may be obtained. When the spirit is used in the place of turpentine, the varnish-maker uses it in the same way, and in the same quantity, as there appears no manner of difference in the use of it from the spirit of turpentine in the making of varnish. When the asphaltum is used, it supplies the place of real asphaltum, and in about the same quantity. I have explained the whole as clear as I can, but if any more information is required, I should feel happy in giving it, and am,

Rectification of the spirit.

Asphaltum.

Pitch.

Sir, With great respect,

Your obedient humble servant,

B. COOK.

*** To such persons as wish for further particulars on the subject of lighting apartments with gas, it may be proper to note, that the society, in their 26th volume of Transactions, page 202, have given an engraving and description of a gazometer, and apparatus for making carbonated hidrogen gas from pit coal, which communication was sent to them by Mr. S. Clegg, of Manchester *.

Lighting apartments with gas.

* See Journal, vol. xxiii, p. 85. See also two original communications by Mr. Cook on the advantages of coal gas lights, even on a very small scale: vol. xxi, p. 291, and xxii, p. 145.

III. *Method*

III.

Method of procuring Turpentine and other Products from the Scotch Fir, (Pinus Silvestris Linn.) *My Mr.* H. B. Way, *of Bridport Harbour* *.

SIR,

Extraction of turpentine from the Scotch fir in this country.

THE enormous high price of turpentine, tar, and pitch, last year, brought to my remembrance, that I had, in 1792, when in America, made some memorandums on the subject of obtaining them in North Carolina, which, on referring to, led me to think, that they might be obtained in this country. I was induced to mention it to my relation and friend, John Herbert Browne, Esq., of Weymouth, and of Sheen, in Middlesex, when on a visit at my house; and I expressed a wish, that I could try the experiment with regard to turpentine; when he very kindly gave me leave to try it on three trees growing on his estate, about three or four miles from this place, and he went with me and fixed on them, and early in last April I had them prepared for the purpose of extracting the turpentine, and they have been running till the 18th instant. The weather, except the last month and part of this, has, from so much rain falling, and there being so little hot weather, been particularly unfavourable for this business; as, the distance being such as to prevent the trees being regularly attended, the hollows were frequently found by my men full of water, and a good deal of the turpentine, which ran off with the water, lay on the ground. Under all these circumstances I was only able to obtain from the three trees about two pounds and a half of turpentine. Mr. Browne being with me again the 16th and 17th instant, as he wished to take the trees down, I begged he would allow me to take a part from one of them, for the purpose of sending to the Society of Arts, Manufactures, and Commerce, with the turpentine collected from

* Trans. of the Soc. of Arts, vol. xxviii, p. 86. The silver medal was voted to Mr. Way. Part of the tree, from which the turpentine was extracted, is preserved, along with some of the products, in the society's repository.

the

the trees, which he most readily complied with. I have therefore taken about six feet from one of them, (they were all nearly the same size); what I have sent is the part from the ground to the top of the place that has been cut away for the turpentine to run into the hollow, whence it was to be collected; the hollow was cut in this considerably higher than is usual in America, as this tree stood in a hedge, and could not well be hollowed lower; I have matted up this part of the tree, and secured it with straw and a double mat, to prevent the bark being rubbed off, that it may be seen in the same state as it stood when the turpentine was taken from it. The turpentine is in the cask in which it was deposited when brought from the trees; and I have this day shipped both on board the sloop Betsey, Captain Trent, bound to Downe's wharf, London, directed to you, freight paid here by me; which vessel I expect will sail in a day or two, and I hope you will receive them safe, which, when you do, you will much oblige me by requesting, that both may be examined, in the hope that this small trial may meet with the approbation of the very highly respectable and truly useful Society of Arts, Manufactures, and Commerce; and if considered likely to prove useful, that they may induce some person, who has the means and opportunity of doing it, to make a trial on a larger scale, so as fairly to ascertain whether turpentine can be obtained in this country from the very large and numerous plantations of Scotch firs, now in the United Kingdom, previous to the trees being cut down, either to thin plantations, or where ground is designed to be cleared, as taking the turpentine from the trees previous to their being cut does not at all injure the wood, and by making the hollow in the trunk of the tree about six inches from the ground, it would waste but a very small quantity of timber. I have taken the liberty of annexing a copy of memorandums I made when in North Carolina, respecting the modes of collecting turpentine, and making tar and pitch, in hopes they may afford the society some little information, as they are not, I apprehend, very generally known. They are copied from memorandums which I actually made on the spot. I would have sent the memorandum

Extraction of turpentine from the Scotch fir in this country.

randum-books with this, had not the remarks been mingled with others relative to my commercial pursuits; but I shall have no hesitation in allowing any person to examine them, or to afford any information in my power to any persons willing to make experiments in this way, if they will favour me with a call. I am well satisfied in my own mind, that very large quantities of tar might be obtained from the knots and limbs of the Scotch fir when cut down; and that the charcoal made from it would not be injured by the tar being first extracted: and as I was in Norway, Sweden, and Russia, in 1789 and 1790, and saw no tree, from which I consider that tar could be extracted, except the Scotch fir, or red deal, which is one and the same tree, I am persuaded, that the refuse of that tree must be what they make the tar from in those countries, though I had no opportunity of seeing the process there. I suspect, that the Swedish tar-kilns must be constructed of brick, or some sort of masonry, as the tar brought thence is much clearer, better, and more free from extraneous matters, than that of any other country. I have observed the tar from North Carolina to have frequently a quantity of sand in it, which is easily accounted for, from the soil in which the kilns are made: it would, in the careless way in which they take it out of the hole dug in a sandy soil, be very likely to be mixed with the sand. In the small cask, in which the turpentine is, I have sent a few small red deal knots, from some timber that I have lately taken out of my warehouse, on some alterations being made; the timber from which they are taken has been in the warehouse ever since the summer of 1786, and yet, when these pieces are exposed to a moderate heat, the tar will be seen to exude from them.

Tar might be extracted from the fir made into charcoal.

Swedish tar-kilns.

North Carolina tar.

I remain, Sir,

Your obedient and very humble Servant,

H. B. WAY.

Bridport Harbour, Nov. 27, 1809.

Extracts from Notes taken by Mr. Way.

Thursday, April 12, 1792.

Method of extracting turpentine in North Carolina.

ARRIVED at Wilmington, North Carolina, about one P. M. Observed on the roads the pitch-pines prepared for extracting

extracting turpentine; which is done by cutting a hollow in the tree about six inches from the ground, and then taking the bark off from a space of about eighteen inches above it, from the sappy wood. The turpentine runs from April to October, and is caught by the hollow below. Some of the trees were cut on two sides, and only a strip of the bark left of about four inches in breadth on each of the other two sides, for conveyance of the sap necessary for the support of the tree. A Captain Cook, with whom I had been travelling, informed me, that some trees would run six or seven years, and that every year the bark was cut away higher and higher, till the tree would run no longer, and I observed many that had done running, and they were in general stripped of the bark on two sides, as high as a man could reach, and some were dead from the operation; others did not look much the worse for it. I find the usual task is for one man to attend three thousand trees, which, taken together, would produce from one hundred to one hundred and ten barrels of turpentine.

April 15, 1792.

ON my return from Wilmington to Cowen's tavern, distant about sixteen miles, I was informed, that the master of the house had been a superintendant of negroes, who collected turpentine. I found the information I had before received was not perfectly correct; he told me he attended to six slaves for a year for a planter, and between the 1st of April and the 1st of September they made six hundred barrels of turpentine. The cutting the trees for the purpose of collecting is called boxing them, and it is reckoned a good day's work to box sixty in a day; the trees will not run longer than four years, and it is necessary to take off a thin piece of the wood about once a week, and also as often as it rains, as that stops the trees running. While in North Carolina, I was particular in my inquiries respecting the making of tar and pitch, and I saw several tar-kilns; they have two sorts of wood that they make it from, both of which are the pitch-pine; the sort from which most of it is made are old trees, which have fallen down in the woods, and the sap rotted off, and is

Farther account of it.

Tar made from two sorts of wood:

lightwood, is what they call lightwood, not from the weight of it, as it is very heavy, but from its combustible nature, as it will light with a candle, and a piece of it thrown into the fire will give light enough to read and write by. All the pitch-pine will not become lightwood; the people concerned in making tar know it from the appearance of the turpentine in the grain of the wood. The other sort of wood which is used, after the trees which have been boxed for turpentine have done running, they split off the faces over which the turpentine has run; and of this wood is made what is called green tar, being made from green wood instead of dry.

and wood from trees boxed for turpentine.

Mode of making the tar.

When a sufficient quantity of wood is got together, the first step is to fix a stake in the ground, to which they fasten a string, and from the stake, as a centre, they describe a circle on the ground according to the size they wish to have the kiln. They consider that one, twenty feet in diameter, and fourteen feet high, should produce them two hundred barrels of tar. They then dig out all the earth a spit deep, shelving inwards within the circle, and sloping to the centre; the earth taken out is thrown up in a bank about one foot and a half high round the edge of the circle; they next get a pine that will split strait, of a sufficient length to reach from the centre of the circle some way beyond the bank; this pine is split through the middle, and both parts are then hollowed out, after which they are put together, and sunk in such a way, that one end, which is placed in the centre of the circle is higher than that end which comes without the bank, where a hole is dug in the ground for the tar to run into, and whence the tar is taken up and barrelled as it runs from the kiln. After the kiln is marked out, they bring the wood, ready split up, in small billets, rather smaller than are generally used for the fires in England, and it is then packed as close as possible, with the end inwards, sloping towards the middle, and the middle is filled up with small wood and the knots of trees, which last have more tar in them than any other part of the wood. The kiln is built in such a way, that at twelve or fourteen feet high it will overhang two or three feet, and it appears quite compact and solid. After the

the whole of the wood is piled on, they get a parcel of small logs, and then place a line of turf, then another line of logs, and so on alternately all the way up, and the top they cover with two or three thicknesses of turf. After the whole is covered in this way, they take out a turf in ten or a dozen different places round the top, at each of which they light it, and it then burns downwards till the whole of the tar is melted out; and if it burns too fast they stop some of the holes, and if not fast enough they open others, all of which the tar-burner, from practice, is able to judge of. When it begins to run slow, if it is near where charcoal is wanted, they fill up all the holes, and watch it to prevent the fire breaking out any where till the whole is charred; the charcoal is worth two pence or three pence, British sterling, per bushel. It will take six or eight days to burn a tar-kiln; in some places they burn it at such a distance from the shipping, that they have very far to roll it, and even then sell it at from three and six pence to five shillings British sterling, per barrel, sometimes taking the whole out in goods, but never less than half the amount in goods; from all which it will be reasonably supposed, that tar burning in that country is but a bad trade, as it must be a good hand to make more than at the rate of a barrel a day; the barrels cost the burner about one shilling and three pence British sterling each; the tar makers are in general very poor, except here and there one, that has an opportunity of making it near the water side.

Pitch is made by either boiling the tar till it comes to a proper thickness, or else by burning it; the latter is done by digging a hole in the ground, and lining it with brick, it is then filled with tar, and they set fire to it, and allow it to burn till they judge it has burnt enough, which is known by dipping a stick into it, and letting it cool; when burnt enough they put a cover over it, which stops it close, and puts out the fire. Five barrels of green tar will make two of pitch; and it will take two barrels of other tar to make one of pitch. Method of making pitch.

N. B.—The foregoing observations respecting tar and pitch are copied from a memorandum made by me at Suffolk,

folk, in Virginia, on the borders of North Carolina, April 23, 1792, and are the result of the inquiries and observations I made on the subject whilst in Carolina.

Wilmington, N. C. April 13, 1792.

Pitch pine timber.

IN conversation with a Mr. Hogg, who had been settled there, and at Fayette-ville before the war, I learnt, that pitch-pine timber growing on the sands was the best; and that it was reckoned to be better if cut in the winter, before the sap rises in the tree.

H. B. WAY.

SIR,

Experiments on a large scale recommended.

IT affords me much pleasure to learn, that my communication, on the extraction of turpentine from the Scotch fir, has been thought worthy of the consideration of the society; and it will be highly gratifying to me, if it should induce persons, who have considerable plantations, to try it on such a scale, as to ascertain to what extent it might prove beneficial in this country. The experiment should be tried on trees so situate as to be conveniently examined every day, and the turpentine collected into the hollows removed as often as possible to prevent its being injured, or wasted by the rain. I think, that during the American war, some importations of turpentine were made from Russia and Sweden; and if so, it must have been extracted from what we call the Scotch fir in a colder climate than this.

Venice turpentine.

The article called Venice turpentine, which is brought from Carinthia and Carniola, is extracted there from the larch tree; and it might probably answer to try to produce it from the larch trees grown in Great Britain, in the same way as I have collected the turpentine from the Scotch fir.

The timber improved by it.

Respecting the wood of the Scotch fir being injured, by the extraction of the turpentine from it, I should rather think, that it would, on the contrary, be better for it; as all those who use deals from Scotch fir in this neighbourhood complain, that it is too full of turpentine to work well. The fact might be ascertained, by the piece of timber which I sent to the society; as, if it was wished to preserve that part in which the hollow is made, the back part, or nearly half of the tree, might be sawn into boards without injury,

jury, and these boards might be compared with some from a tree taken down in the winter, from which the turpentine has not been extracted. It must, however, be noted, that from the tree I have sent to the society, the turpentine has only been running one year, whereas, in America, they collect the turpentine from the same tree for three or four succeeding years. It has been supposed and asserted, that turpentine was only obtainable from the United States; but I have sufficient documents to prove, if required, that a very large quantity of it can be procured from East Florida; and I well remember, that about the year 1782, several cargoes of turpentine were shipped in the river St. John's, for *Britain*; and though that country is at present in the hands of the Spaniards, no doubt, arrangements might be made with the Spanish government for a supply of that necessary article thence. It is my earnest wish, that, through the medium of the Society of Arts, I may render any information that may be serviceable to the interest of the united empire; and I will, with pleasure, furnish farther communication on the products of Florida and its commerce, if desired by the society.

Turpentine may be procured in large quantity from East Florida.

I am convinced, that tar might be produced from the refuse of firs of English growth to advantage; and that a much better article might be made from them in Britain, than any imported from America. The Scotch firs, in England, from being planted at a greater distance from each other than they are naturally found abroad, have much larger knots, and greater numbers of them, than in Carolina, or the north of Europe, and would therefore produce more tar, in proportion, from their refuse of wood, than the trees of those countries.

Tar might be obtained from the refuse of British firs.

The pitch-pine of Virginia, the Carolinas, Georgia, and the Floridas, grows to an immense size in what are there called pine barrens, the soil of which is finer and whiter than the sand used as writing-sand in Great Britain, and the trees grow almost to the verge of high-water mark on the sea-shores. I think it would answer a good purpose for the society to encourage, by premiums, the extraction of turpentine from British firs. I remain,

Pine-barrens.

Sir, Your obedient, and very humble Servant,

H. B. WAY.

J. H.

Testimony of Mr. Browne.

J. H. Browne, Esq., of Weymouth, certified, that he had witnessed the principal experiments made by Mr. Way, in extracting the turpentine from the Scotch firs. That the trees had been planted in 1771 or 1772; and that the wood, subsequent to the operation, had been minutely examined, and found not to be injured by the extraction of the turpentine. He added, that the season was uncommonly wet and unfavourable for the experiment.

Reference to the Description of Mr. H. B. Way's Method of procuring Turpentine from Fir Trees. Plate IX, Fig. 4.

a, Represents the lower part of a fir tree, as growing in the earth; *b*, shows the part where a portion of the bark is taken off to assist the emission of the turpentine; *c* is a hollow cut within the body of the tree, it is in the form of a basin at the lower part to receive the turpentine, which exsudes into it from the pores of the tree; this basin is about six inches from the ground.

IV.

Analysis of Deadly Nightshade, Atropa Belladonna; by Mr. Vauquelin*.

Deadly nightshade examined for the acid principle of tobacco.

THE experiments I am about to relate were instituted for the purpose of knowing, whether this plant, which is of the same family as tobacco, contained the acrid principle, that we found in the latter †; but we shall see below, that it does not exist in them. However, I availed myself of this opportunity to examine the properties of the matter in this plant, which, according to the physicians, is narcotic.

The juice

1. The expressed and filtered juice of belladonna has a pretty deep brown colour, and a bitter nauseous taste.

coagulated by heat.

It is copiously coagulated by heat, and by an aqueous infusion of gall.

* Ann. de Chim. vol. lxxii, p. 53. † See Journ. p. 260.

2. The

2. The substance coagulated by heat in the juice of belladonna is of a yellowish gray, becomes black by dessication, and presents a smooth polished fracture like that of resins. It burns with decrepitation, softening, and smoke, which has the same smell as horn similarly treated. The coagulum.

3. The juice of belladonna, distilled till it is reduced to the consistence of a liquid extract, yielded a water, which had only a flat herbaceous taste, and nothing of the acrimony of that of tobacco. The only reagent, of all we tried, that rendered it slightly turbid, was acetate of lead. The juice distilled.

4. The juice reduced to the consistence of an extract being treated with alcohol, part dissolved in it; and the solution deposited on cooling crystals of nitrate of potash, and a little muriate of the same base. The residuum treated with alcohol.

The alcohol, separated from these crystals and evaporated, left as a residuum a brownish yellow matter, of a very bitter and nauseous taste; which, taken up a second time by highly dephlegmated alcohol, left a fresh quantity of insoluble matter, and still deposited a few crystals of the same salt. Spirituous extract.

The matter being divested as much as possible by this process of the greater part of the nitre, and of the substance insoluble in alcohol, I evaporated the alcohol, and subjected its residuum to the following experiments:

1. It dissolves abundantly and speedily in water, and is even deliquescent in the air. Its properties.

2. The solution is of a yellowish brown, and has a very bitter and very disagreeable taste.

3. It reddens litmus paper very deeply.

4. It is copiously precipitated by alcoholic tincture of galls, and not by acetate of lead, if the latter be sufficiently diluted with water; but, as it contains a little muriate of potash, it would precipitate the acetate of lead without this precaution.

5. This solution, when mixed with sulphuric acid, emitted a very evident smell of acetic acid.

6. The same solution, on the addition of nitrate of silver, threw down a true muriate of silver.

7. Caustic potash produces from the solution a fetid smell very similar to that of stale lie, in which linen has been washed,

washed, and which is beginning to putrefy. Ammoniacal vapours too arise, which may be rendered sensible by weak nitric acid held at a little distance from the mixture.

8. The addition of a few drops of sulphate of iron renders the solution of a much deeper colour.

9. The extract itself, exposed on burning coals, swells up, and emits pungent and acrid fumes, in which the smell cannot distinguish ammonia.

Its contents.

From the effects produced on the solution of extract of belladonna by the various tests employed above we may conclude, that it contains, 1, a free acid; 2, an alkaline muriate; 3, a small quantity of an ammoniacal salt.

The acid the acetic.

The acid that exists in it must be the acetic, since sulphuric acid elicits the smell of this acid, and acetate of lead occasions no precipitate; which it would, if the acid were the malic, tartarous, or oxalic. Part of this acid must be combined with potash; and it is this, no doubt, that communicates to the extractive mass the property of attracting the moisture of the air.

Its poisonous qualities not owing to the salts or acid.

But neither these salts, nor these acids [this acid], impart to the matter its poisonous qualities. These unquestionably reside in the vegetable substance itself. What then is the order of composition, that makes thus, with the same principles, both our food and such deadly poisons? This is one of the barriers, that chemistry has not yet been able to overstep; and unfortunately beyond this barrier lie secrets of the utmost importance to mankind. Wanting therefore the means, which at some future period may give us a precise knowledge of the differences, that exist between vegetable compounds possessing such opposite properties, we must have recourse to observation of their effects.

Destructive distillation of the extract.

One of the methods, that appeared to us best adapted to elucidate the nature of that substance in belladonna which is soluble in alcohol, was its decomposition by fire. Accordingly I introduced 2·7 gr. [41·7 grs.] into a glass retort, and heated it gradually, till the water of solution had been distilled over by a very strong heat. A yellow ammoniacal liquid passed over, and afterward a thick oil, which had a very singular disagreeable smell.

On

On examining the liquid product I found a great deal of free ammonia, though there was some in combination, for the addition of a few drops of caustic potash rendered the ammoniacal smell much stronger. The oil was black, very thick, and very acrid.

The coal. The coal left in the retort weighed 1 gr. [15·45 grs.]. It had an alkaline and prussiate taste. Washed with boiling water it yielded a lixivium, which, being mixed with sulphate of iron, produced a very considerable quantity of prussian blue, considering the small quantity of matter employed. The coal, after having been lixiviated and dried, still weighed 7 dec. [10·81 grs.].

Its quantity very great. This quantity of coal, exclusive of what was encrusted on the retort by the violence of the fire, which I was unable to separate, is larger than is furnished by most other vegetable matters, that I have yet had an opportunity of distilling: for the 2·7 gr. [41·7 grs.] certainly contained more than 7 dec. [10·81 grs.] of water, beside nitrate and acetate of potash.

It contains much hidrogen and nitrogen. It appears too, that it contains a large quantity of nitrogen and hidrogen, since it yielded by distillation a great deal of ammonia, prussic acid, and oil. But as this matter might contain a little nitrate, I suspected, that part at least of the nitrogen forming the ammonia and prussic acid was produced from the nitric acid.

Gum arabic mixed with nitre and distilled. To clear up this doubt, I mixed 6 gr. [92·67 grs.] of gum arabic, in which there is supposed to be nitrogen, with a tenth of saltpetre; and, after having distilled, examined the products. The liquid that came over was in fact ammoniacal; and its smell became stronger on the addition of potash, which shows, that an acid was formed at the same time with the alkali.

The coal remaining in the retort, which weighed 2 gr. [30·89 grs.], and was extremely pyrophoric, contained prussiate of potash, like that of my matter. But though I employed in this experiment three times as much gum, and probably more saltpetre, the mixture did not furnish so large a quantity of ammonia, or of prussic acid, as the nauseous principle of belladonna.

 If

Much prussic acid and ammonia furnished by the extract.

If we admit therefore, that the saltpetre contained in the 2 gr. of this principle gave rise to some prussic acid and ammonia, we ought not to infer, that the vegetable matter in question furnished none. That it did is the more probable, because its solution was precipitated by infusion of galls. Be it as it may, this experiment shows, that it is difficult to judge from distillation, whether organic matter containing saltpetre be of a vegetable or animal nature.

It abounds in combustible radicals,

The results of this analysis, though hitherto very rude, are sufficient however to show, that the substance, which constitutes the subject of them, contains a great deal of charcoal, hidrogen, and nitrogen, and but little oxigen, if we may judge from the small quantity of carbonic acid formed during its decomposition by fire.

which are probably the cause of its effects,

From what has been said may we be allowed to suppose, that the narcotic effects, which belladonna produces in the animal economy, are owing to the superabundance of combustible radicals, and particularly to that of the charcoal over that of the oxigen in the principle of this plant soluble in spirit of wine? Without pretending to assert this, it is nevertheless certain, that all the vegetable matters, which produce analogous effects, are rich in charcoal, hidrogen, and nitrogen, while substances greatly oxigenated produce contrary effects.

particularly assisted by nitrogen.

It must be confessed too, that a great many vegetable products equally abundant in these two principles do not possess the same qualities; but the nitrogen, which is always found associated with hidrogen and charcoal in the somniferous plants, does not exist, at least in similar quantity, in the others.

Examination of the part of belladonna insoluble in alcohol.

Part insoluble in alcohol examined.

1. This matter dissolved in water communicates to it the property of frothing when shaken.

2. The solution is copiously precipitated by aqueous infusion of galls.

3. Nitrate of barytes causes in it a precipitate partly soluble in nitric acid.

4. Muriate

4. Muriate of lime produces a precipitate wholly soluble in nitric acid.

5. The solution reddens litmus paper.

6. Nitrate of silver produces in it no effect.

7. Burned in a crucible it leaves an alkaline and hepatic coal.

From these effects we may conclude, that this part of belladonna is composed of an animal matter, of sulphate of potash, of acidulous oxalate of potash, probably of nitrate, and that it contains no muriate. Its component parts.

We may conclude too from these effects, that no earthy salts are present in it, since muriate of lime, as well as nitrate of barytes, produces in it a precipitate.

I satisfied myself by trials made on a larger scale, that the precipitates occasioned in the solution of the substance in question by nitrate of barytes were, the first, oxalate of lime, the second sulphate of barytes.

The oxalate of lime had carried down with it a large quantity of animal matter, which gave it a brown colour. This indicates, that this salt has a powerful attraction for animal matter; and explains why mulberry calculi, which are known to be composed of oxalate of lime, have a much deeper colour than other calculi. Oxalate of lime has a strong attraction for animal matter.

After having precipitated successively, as I have said, the sulphate of potash, and acidulous oxalate of potash, I evaporated the liquor, which was still coloured, and contained nitrate of potash and muriate of lime; and I treated it with nitric acid, to know whether it contained any gum: but, as I could not obtain an atom of sacchlactic acid, I concluded, that it contained none. It was formed only of oxalic acid and a yellow matter. This substance appeared then to be entirely of an animal nature. No gum in it.

From what has been said we find, that the juice of belladonna contains the following matters: Matters contained in the juice of belladonna.

1, An animal substance, which is partly coagulated by heat, and partly remains dissolved in the juice by means of the free acetic acid present in it.

2, A substance soluble in spirit of wine, which has a bitter and nauseous taste, by combining with tannin becomes insoluble, and furnishes ammonia when decomposed by fire.

 3, Several

3, Several salts with base of potash, namely, a great deal of nitrate, some muriate, some sulphate, acidulous oxalate, and acetate.

The woody part.

The substance of the belladonna, from which the juice had been expressed, having been washed with hot water, dried, and then burned, left ashes composed of a pretty large quantity of lime, phosphate of lime, iron, and silex.

This lime announces, that the plant contained oxalate of lime, which had been decomposed by the fire.

Its properties owing to the part soluble in alcohol.

There can be no doubt, that it is the matter in belladonna soluble in alcohol, which alone produces its deleterious effect on the animal economy; for it is the only sapid part, and the well known effects of all the other matters accompanying it in no respect resemble those of the plant.

This proved on a dog.

To place this beyond doubt, I gave a middle-sized dog a certain quantity of this principle mixed with his food.

Experiment 1.

A quarter after twelve I made this dog take 1 gr. [15.45 grs.] of the extract rolled up in 10 gr. [154·5 grs.] of bread and meat made into a paste.

Effects.

In three quarters of an hour the animal appeared inclined to sleep; he held his head down, and seemed unable to support it; he lay down several times with his head on the ground; his paws were slightly convulsed; his jaws moved for some time, as if he were chewing. These effects continued about three quarters of an hour, but nothing farther ensued, and the dog resumed his ordinary manners.

Experiment 2.

At 2 o'clock in the afternoon I gave him 2 gr. [30·89 grs.] of the extract in 12 gr. of paste. The effects were renewed; but they were slighter, and of shorter duration.

Experiment 3.

At 3 o'clock I made him swallow 4 gr. [61·78 grs.] of the same extract, with about 30 gr. [363 grs.] of paste.

Effects.

A few minutes after he was seized with a continual but uncertain and difficult movement; chiefly in the abdominal extremities; and uttered some plaintive cries.

At half after three he found great difficulty in moving himself; he dropped frequently on his hind feet; and his respiration was very much confined. He attempted several times to go through the wall, which showed a sort of delirium. He had then a trembling in all his muscles.

At

At a quarter after four he lay down, and appeared in a profound sleep. His pulse was too quick to be counted.

At half after four he vomited up the paste he had taken; and some time after he rose, but walked with difficulty, falling sometimes on one side, sometimes on his hind legs.

He carried his head very low, his eyelids drooped, and he no longer distinguished objects; at least he struck himself against the walls and furniture of the laboratory in walking. His nostrils were scarcely sensible to the vapour of ammonia; and his ears heard nothing, for the most sudden noise did not make him stir.

He had not lost his memory however; for having put him, with a view to give him some vinegar and water, into the same posture as when he took the paste, he flew into a dreadful rage, as if all his strength had been at once renewed. From that time the symptoms he had experienced imperceptibly diminished, and about 8 o'clock at night he had recovered all his outward senses, but was still greatly fatigued. The next day he ate as usual.

Such are the phenomena this animal exhibited: and every one must perceive in them the effects of narcotism and intoxication carried to their extreme; whence resulted a sort of delirium. It is probable, that if he had not brought up the greater part of the matter, before it had time to produce its effect, it would have killed him. Narcotic and intoxicating.

V.

On the Use of Sulphate of Soda in the Fabrication of Glass: by Mr. MARCEL DE SERRES, *Inspector of Arts, Sciences and Manufactures* *.

MY object is to give some account of the attempt made by Dr. Gehlen to employ the sulphate of soda in glass-works; and as I have had an opportunity of seeing the results of his experiments, and conversing with him on the subject of those, which he still intends to make, on the different Sulphate of soda employed in glass-making.

* Abridged from Ann. de Chim. vol. lxxvi, p. 172.

different substances that may be employed in glass-houses, I conceive, that the following particulars will not be uninteresting. They who wish for information more at large may find it in the work which Dr. Gehlen has lately published, entitled *Beytrage zur wissenschaftlichen Begruendung der Glasmacherkunst*, Attempt to establish the Art of Glassmaking on Scientific Principles, Munich, 1810.

From a number of experiments, made in the large way by Mr. Francis Baader and Dr. Gehlen, it appears;

General observations.

1, That sulphate of soda perfectly freed from its water of crystallization, may be very successfully employed in manufacturing fine white glass, without the addition of potash or soda.

Advantages.

2, That in using this flux there is a considerable gain in point of time; and consequently in the product of a given furnace, and in materials. These advantages arise from a larger quantity of silex being dissolved by sulphate of soda freed from its water of crystallization.

Requires considerable care.

3, That it only requires great accuracy in the addition of the quantity of charcoal necessary to effect the decomposition of the sulphate of soda. This is so essential, that sometimes a single hundredth part too much, or too little, almost spoils the vitrification, or colours the glass. It must be observed too, that it is difficult to give precise directions for the quantity of charcoal to be employed, because the proportion must vary according to its dryness or moisture. If it be moist, it will yield more carbonic acid, which cannot certainly be advantageous to the vitrification.

Should be first decomposed into a sulphuret.

4. That sulphate of soda cannot be employed so well in substance in the melting pots; but that it is better first to make a sulphuret of soda, in order to get rid of the large quantity of carbonic acid, which is formed in the disoxidation of the sulphuric acid, and would cause too great an effervescence in the melted matter.

Glass-gall.

5, That the glass-gall is decomposed by an addition of charcoal in all the other manufactures of glass, which is a great advantage, because this gall is the greatest enemy to the manufacture of fine glass.

Peculiar attention to the pots.

6, That the pots, in which the glass is melted by means of sulphate of soda, must be made with much care, and with

with a different proportion of materials, because this glass attacks them much more than that made with potash.

7, That sulphate of soda may be very well prepared by decomposing muriate of soda; and for this purpose the waste of vitriol manufactories may be employed, which is a considerable saving. Preparation of the sulphate.

8, Lastly, it is well known, that, when fine glass is made, and more soda or potash is mixed in it than in common glass, the glass, if not properly cooled before it is wrought, though at first very pure, begins soon to enter into fermentation while working, and afterward appears full of blebs. It is observable, that glass made with feldspar containing potash always abounds in blebs; yet it is possible, to make good glass of it, and thus turn to account the potash contained in it. Blebs in glass.

Experiments.

As the sentiments of Kreschmann, Pott, Laxmann, Gren, Lampadius, Van Mons, and Pajot-Descharmes, respecting the use of sulphate and muriate of soda in the fabrication of glass, differ widely, it was necessary to make the following experiments, to ascertain the processes, that might answer. Experiments.

1. First a mixture of quartz and sulphate of soda, in the proportions of 100 to 60, was made, and exposed to the fire of a glass-house furnace twenty-two hours. At the end of this time no virtification had taken place, or at least it was imperfect, however high the heat was carried. Experiment 1.

2. Quartz, sulphate of soda, and burnt lime, were taken in the proportions of 100, 100, and 15, and heated. A second mixture was made in the proportions of 100, 50, and 20; and a third in the proportions of 100, 54, and 17. The third mixture was heated in a furnace the fire of which was urged by bellows. At the expiration of four hours more vitrification had taken place, it is true, than in the first experiment; but the glass was very stiff, and as it were stony. Experiment 2.

3. Quartz, calcined potash, lime, and sulphate of soda, were mixed in the proportions of 100, 10, 17, and 43, and Experiment 3.

at

at the expiration of an hour and a half the result was the same.

Experiment 4. 4. Quartz, sulphate of soda, lime, and charcoal dust were mixed in the proportion of 100, 54, and 14, for the former three; and the charcoal was varied from 4 to 4·2, 4·4, and 4·5. These mixtures were left in the fire an hour, and a brownish yellow or sometimes colourless glass was obtained, the colour always depending on the proportion of charcoal employed.

Experiment 5. 5. In the 5th experiment quartz was mixed with sulphuret of soda, obtained from carbonate of soda and sulphur heated together till no more sulphur was sublimed, in the proportion of 100 to 60:

Experiment 6. 6. In the 6th quartz was mixed with sulphuret of soda, obtained from eight parts of calcined sulphate of soda and one of charcoal dust, and lime, in the proportions of 100, 45, and 17:

Experiment 7. 7. In the 7th quartz was mixed with sulphate of soda, sulphuret of soda, and lime, in the proportions of 100, 24, 24, and 17; and also in those of 100, 2·5 or 3, 45, and 17. The mixture was left in the fire an hour. These experiments gave the same result as the 4th. When these trials, and many more, the particulars of which it is unnecessary to recite, had been made, the process was attempted in the large way. The mixture was formed of 100 parts quartz, 54 sulphate of soda, 17 lime, and 5 charcoal. During the fusion, a shovelful of burning charcoal from the furnace was thrown in, the five parts proving too little in the circumstances, that took place in the glass furnace. The general results of these experiments were:

General results of the experiments. 1. That sulphate of soda may be employed in glassmaking, without any addition of potash or of soda. The glass obtained by this process is as beautiful and as white as glass made with the usual materials, and has all the same qualities.

2. That the vitrification of sulphate of soda with quartz is very imperfect even in the strongest fire. It is more complete, if lime be added, but then it requires a great deal of time and fuel: and it is rendered perfect by the help of a substance, that decomposes the sulphuric acid of the

the sulphate of soda, and thus removes the obstacle, that prevents the soda from acting on the silex. The best medium that can be employed is charcoal, or for flint glass metallic lead.

This decomposition may be conducted during the vitrification, or previous to it. The methods employed must be varied according to circumstances, but it is essential to observe, 1st, the property charcoal has of colouring glass, even when in very small quantity; this property of charcoal not being exceeded by any of the metallic oxides hitherto known: 2dly, the preference to be given to lime reduced to powder, dissolved in water, and heated anew, before lime slacked in the air: 3dly, the great effervescence of the glass, when sulphate of soda is employed, an effervescence, however, not greater, than sometimes arises from common soda; and hence the precaution that must be taken to add it in smaller successive portions, than if potash were employed: 4thly, that the work must be carefully distributed in glasshouses of this kind, not to be troubled by this effervescence: 5thly, that sulphuret of soda may be more useful in glassmaking than sulphate of soda: and lastly, care must be taken in preparing the pots, because the sulphate of soda has a particular effect, as every other flux has. Precautions necessary.

VI.

On the Cause of the Refrigeration observed in Animals exposed to a high Degree of Heat: by FRANCIS DELAROCHE, *M. D.* *

THE animal economy presents us with phenomena, which, differing in their nature from those exhibited by inorganic bodies, cannot be explained by the ordinary results of the laws of physics; while at the same time it produces others, which, being more or less similar to physical effects, are apparently derived from the same laws. Some physiologists, it The animal economy subject to peculiar as well as physical laws.

* Journal de Physique, vol. lxxi, p. 289. Read to the Institute the 6th of November, 1809.

Mechanical philosophy has been carried too far.

it is true, struck with the errours committed by those, who have had a rage for ascribing every thing to mechanical laws, will not admit any explanation of this kind in the animal economy. They are of opinion, that the phenomena, essentially connected with the exercise of life, must depend on the laws that govern vitality; and not on physical laws, which have little apparent connection with the former, and very frequently seem in opposition to them. But is not this opinion founded on reasoning rather than experiment? And if some of the phenomena of life appear to be contradictory to those laws, to which inanimate bodies are subject, must we thence infer, that it is the same with all of them? This reasoning, erroneous in itself, would be contradictory to experience. Who, indeed, can overlook the influence of physical causes in several of the phenomena of the animal economy; such for instance as distinct vision, which depends essentially on the refracting powers of the humours of the eye; or the movements of our limbs, in which our bones act as levers, our tendons as cords? It is true, that physical causes alone are not sufficient to produce these results, and vital causes* powerfully concur in them; but the influence of the former is not the less evident. Generally speaking it may be said, that there is scarcely a phenomenon of the animal economy, which is not owing to both. Sometimes the influence of physical causes is predominant, at others that of the vital; and frequently it is difficult to determine with precision what belongs to one, and what to the other. It is of no small consequence, however, to attain this object; and the researches capable of leading to it may be ranked among the most important in physiology. If we can ever hope to ac-

But not wholly to be rejected.

Commonly both act:

sometimes one predominating, sometimes the other.

Vital causes.

* When I speak of vital causes and vital laws, I do not mean to assert, that they are actually different from the general laws, that govern inanimate matter, and independent of them: they are, perhaps, only modifications of them; but I am of opinion, that, in the present state of science, we must admit them, if we would acquire tolerably accurate ideas of the mode, in which the different functions of organic bodies are executed. We are yet far from having reached the period, when many of the phenomena exhibited by these bodies may be referred to the laws of physics.

quire

quire precise notions of the vital powers, and how they differ from physical, it must be by observing what is peculiar to them in the vital functions, not by vaguely ascribing to them all the phénomena of organic bodies.

One of the phenomena, in which it seems to me most easy to make the distinction, is that exhibited by animals exposed to a high degree of heat. It is well known that they then assume a temperature much below that of the surrounding medium. It is near half a century since this remarkable faculty in animals was noticed; and it has subsequently given rise to various experiments, particularly those made conjointly by Sir J. Banks, Sir C. Blagden, Dr. Fordyce, and some other philosophers: but we have not yet any precise ideas of its cause, which some suppose to be the refrigeration produced by the evaporation of the perspirable matter, others the same with that of animal heat, whether they imagine themselves acquainted with this, or believe it to be yet unknown. Some considerations on this question will form the subject of the present paper: but I think it necessary in the first place to repeat an observation, which I made some years ago*; this is, that we form a greatly exaggerated notion of this phenomenon, when we suppose the faculty of producing cold in animals is as striking, as that of producing heat. I believe I have proved, that this opinion, which has generally prevailed since the publication of the experiments abovementioned, is altogether erroneous. In fact, in a number of experiments made in common with my friend Dr. Berger, I constantly found, that the temperature of animals exposed to a higher heat than 35° or 40° cent. [95° or 104° F.] rose in a very striking degree, without however reaching that of the surrounding medium. I frequently observed, that this rise of temperature amounted to 6° or 7° [10·8° or 12·6° F.]; and I even ascertained, that, when the external heat is very considerable, this increase of temperature has

Animals exposed to a high temperature generate cold,

but not in such a degree as is commonly supposed.

Their heat increased

* In my inaugural dissertation, entitled Experiments on the Effects that a high Degree of Heat produces in the Animal Economy. See, Collection of Theses of the Medical School at Paris, for 1806, No. 11.

no

without limit. no limit but the death of the animal, which is its necessary consequence. In these experiments I ascertained the temperature of the animals by a method, of the accuracy of which there can be no doubt; that is, by introducing a considerable way into the rectum the bulb of a thermometer purposely made very small. I found a similar increase of temperature in the human subject, by means of a thermometer introduced into the mouth: and I even observed it very strikingly in a case, where the head could be affected only by means of the circulation; that of a person placed in a box filled with hot vapour, but having his head out of it.

A man in hot vapour with his head free.

The faculty of producing cold real.

It follows from these facts, that the faculty of producing cold is much more limited than is commonly supposed; not that it is imaginary. Too many facts attest its existence, for it to be doubted: it is desirable, therefore, to ascertain its cause; and this I shall attempt to do.

Its cause supposed to be the same with that of producing heat.

I have said above, that some suppose this cause to be the same with that of animal heat; and they ground this opinion on the results of the experiments of Blagden and Fordyce, from which it would seem we may infer, that animals preserve a uniform temperature, whatever may be the heat of the surrounding medium; and that consequently their faculty of producing cold is as decided as that of producing heat. Indeed, if it were thus, it would be natural to conceive this uniformity of temperature as one and the same phenomenon, originating from a single cause: but this not being the precise fact, as I have shown, we may presume the conclusion to be erroneous.

This is a mistake.

Proof from a similar faculty in cold blooded animals.

There is one observation, that tends strongly to support this opinion. It is, that cold-blooded animals possess the faculty of preserving a temperature below that of the surrounding medium, when this is elevated, as much or more than warm-blooded animals: though, if this faculty arose from the same cause, as that which produces animal heat, cold-blooded animals should be nearly destitute of it. The truth of this assertion I have shown by several experiments in the paper already quoted; and the following, which I lately made, appear to me to render it unquestionable.

In

In a stove I exposed to a mean temperature of 45° [113° F.] a rabbit, the temperature of which before the experiment was 39·7° [103·46° F.]. After remaining there an hour and forty minutes, it had acquired a temperature of 43·8 [110·84° F.]. A frog, exposed in the same stove to a similar heat, acquired in one hour a temperature of 26·7° [80·06° F.]; which it preserved during the rest of the time it remained in the stove, being half an hour. The temperature of another frog, exposed to a mean heat of 46·2° [115·16° F.], rose to 28° [82·4° F.], at which it became stationary.

Experiments on a rabbit and two frogs.

They who have imagined, that there was no necessary connection between the cause of animal heat, and that of the cold sometimes produced in the animals, have supposed, that the latter might be occasioned by the evaporation that takes place, either at the surface of the body, or in the lungs; thus comparing this phenomena with the cooling of inanimate bodies, the surface of which is wet. For this ingenious comparison we are indebted to Franklin; but is it just? The only experiments made till lately, with a view to solve this question, those of Sir C. Blagden and his coadjutors, and those of Dr. Crawford, seem to indicate, that it is not. Those I made myself a few years ago, and of which I have given an account in the thesis already quoted, led me, on the contrary, to adopt the supposition of Dr. Franklin; though they did not allow me to form a decisive opinion. I have since attempted some new ones, which, confirming the results I had before obtained, appear to me calculated to remove all doubts on the subject. Of these I shall give the results preceded by a brief account of those I formerly published.

Some have supposed the cold produced by evaporation.

This contradicted by some experiments; confirmed by others.

The principal object of the latter was to ascertain the validity of the objection commonly made to Franklin's theory, that the cooling produced by evaporation is insufficient, to explain the difference observed between the temperature of animals exposed to a high degree of heat and that of the surrounding medium. To determine this it was sufficient, to examine the comparative influence of heat on the temperature of animals, and on that of inanimate substances wetted all over. For this purpose I exposed at the same

The insufficiency of this cause has been alleged.

Comparative experiments.

same time, and side by side, in a stove, various animals, alcarrazas filled with water, and wet sponges. In making this experiment, which I have several times repeated, I constantly observed, that the alcarrazas and sponges, whether I introduced them into the stove cold, or previously warmed, assumed a temperature below that acquired by the warm-blooded animals, but nearly the same as that of the cold-blooded *. From these results then we may infer, that evaporation is sufficient, to produce a refrigeration as great, if not greater than that observed in animals; and hence we may presume, that it is the cause of the latter. It would be wrong however, to consider the latter as a necessary consequence of the preceding proposition. The possibility of a thing is not a sufficient ground for our concluding, that it actually is. Accordingly, when I publish-

Evaporation an adequate cause.

* To render the experiment completely accurate, it would have been necessary to ascertain the final temperature, that would have been acquired both by the animals and the inanimate substances, when the heat had produced its utmost effect on them. This I found very difficult with respect to warm-blooded animals: a long continued heat exhausting them greatly, I satisfied myself with an approximation to the limit. I generally waited, till the inanimate substances had attained it; which was much more easy, because I took care previous to the experiment, to raise their temperature nearly to the point, at which it would ultimately arrive by their exposure in the stove.

I shall here give the result of two experiments of the same kind, lately tried.

Results of experiments.

I enclosed in one basket, separating them only by an open partition, a rabbit, and an alcarraza full of water; and placed them in a stove, the mean heat of which, during the experiment, was 45° [113° F.]. The temperature of the rabbit, when introduced, was 39·7° [103·46° F.]; that of the alcarraza about 35° [95° F.]. The temperature of the rabbit gradually rose to 43·8° [110·84° F.]; that of the alcarraza, on the contrary, fell to 31·4° [88·52° F.], at which point it appeared to continue stationary.

In the second experiment I exposed, in the same stove, to a mean temperature of 36·5° [97·7° F.], two small sponges and a frog. The latter, which was placed between the two sponges, acquired, in the course of an hour, the stationary temperature of 28·2° [82·76° F.], the sponge on the left that of 27·9° [82·22° F.], and the sponge on the right that of 27·6° [81·68° F.].

ed

ed these results, I did not pretend to decide, that evaporation was the true cause of the phenomenon in question; I merely held out this opinion as plausible: at present I think I can give direct proofs of its justice.

If evaporation be the sole cause that produces the refrigeration of animals exposed to a high degree of heat, it is evident, that by suppressing it both on the surface of the body and in the interior of the lungs, this refrigeration will be prevented, and the animals must acquire a temperature equal or superior to that of the medium, in which they are immersed. If we do not obtain this result, it is a proof of the insufficiency of this cause: if, on the contrary, the means employed for suppressing evaporation being such as not to disturb the exercise of the other functions of the animal, we perceive a cessation of the phenomenon, for which we are endeavouring to account; we may conclude, with equal reason, that it was owing to evaporation. Test of its efficacy.

This mode of ascertaining the influence of evaporation in this phenomenon naturally offered itself to the minds of those, who have instituted inquiries on the subject. Some experiments have been tried with this view; but they have neither been numerous, nor very decisive. One was by Dr. Fordyce. This gentleman, having introduced a large quantity of aqueous vapour into a heated room, thought he perceived, that the heat incommoded him more, but that his temperature remained scarcely the less stationary. It is to be observed, that the time he passed in this room was too short, to heat very perceptibly a mass so considerable as that of the human body. No positive inference therefore can be drawn from this experiment; any more than from that, in which Dr. Crawford attempted to ascertain the influence of a warm bath on the temperature of a dog; the mode in which he measured this temperature being too inexact, and, besides, the effect of the water being capable of suppressing the cuticular evaporation merely, not the pulmonary. The experiments which the same gentleman tried with frogs, animals in which the pulmonary evaporation must be inconsiderable, would be more decisive, if the results he gives were agreeable to observation. But I have satisfied myself, that this is not the case. Numerous experiments, Experiments with this view by Dr. Fordyce and Dr. Crawford. Frogs acquire the tempera-

ture of water in which they are. periments, made with care, have convinced me, that frogs constantly acquire the temperature of the water in which they are immersed, let its heat be what it may, and that in this respect there is no difference between dead and living frogs*.

New experiments made These are all the experiments, as far as I know, that have been tried to ascertain what would happen, when men or animals are exposed to a high degree of heat, without any evaporation being able to take place from the surface of their bodies. Their insufficiency is evident. New ones therefore cannot be uninteresting, and these I have attempted.

on the plan of Dr. Fordyce. With this view I had recourse to the means employed by Dr. Fordyce, but with this difference; instead of trying the experiments with man, I employed animals of small size, that their mass might be more quickly heated. In fact it is well adapted to the end proposed; for it is obvious, that if an animal be placed in air loaded with vapour, there can be no evaporation of the fluid exhaled either on the surface of its body, or from its lungs; and yet it may continue the exercise of its functions as freely as in dry air. The apparatus I employed allowed me to distribute the vapour pretty uniformly throughout the space occupied by the animals, and to regulate the quantity at pleasure. The following is a description of it.

The apparatus described. A A, Pl. X, fig. 1, is a box, about forty inches high, twenty broad, and sixteen deep: divided into two compartments by an open partition, shown at *n n* in the section, fig. 2. At *a a* is a door, sliding in a groove, and opening into the upper chamber; in which is a circular wicker cage, *i i* fig 2, with a door opening opposite that of the outer box. In this cage the animals were placed. A thermometer, with a very long stem and small bulb, is fixed in the centre of this cage. It is protected from injury by an open wicker case, *m m* fig. 2; and reaches above the top of the box, fig. 1, at *b*, where it is graduated. The vapour is generated in a small tin boiler C C, heated by means of the

* See my Essay on the Effects produced in the Animal Economy by a high Degree of Heat, p. 54 and foll.

furnace

furnace B B; and is conveyed into the box by a tube *c c*. Over the aperture of this tube, within the box, is placed at a little distance a plate *l l*, fig. 2, about four inches square; which prevents its ascending in a direct current to the top, and causes it to be distributed pretty uniformly throughout the apparatus. Toward the bottom of the tube of communication *c c* is a cock, so constructed, that the vapour may pass through a lateral opening, or through the tube itself, or through both at once. This allows the quantity of vapour admitted into the apparatus, and consequently the temperature, to be regulated at will. This is the more easy, as by means of a very simple contrivance, the key of the cock may be turned, without losing sight of the thermometer, that indicates the temperature of the box. To the key of the cock is affixed a pretty long lever, *e e*, from each end of which a string passes to the corresponding end of a lever of the same length, *d d*, turning on a pivot fixed on the top of the box. Of course by moving the lever *d d* a similar motion will be given to *e e*. A section of the cock is given at fig. 3. In the top of the boiler C C is a funnel with a cock, by means of which it can be replenished with water when necessary.

Into this apparatus I introduced successively warm-blooded animals of different kinds, and frogs: I exposed them to different degrees of heat: and I carefully examined their temperature, both before and after the experiment, by means of a thermometer introduced into the rectum, or plunged deep into the œsophagus. The results I obtained are given in the following table. Experiments.

 Table

Results of the experiments.

TABLE of Results obtained on exposing various Animals to a moist Heat, in order to determine the Influence of this Heat on their Temperature.

Exp.	Animal.	Time of remaining in the box filled with vapour.	Mean temperature of the apparatus during the continuance of the experiment.		Temperature of the animal after its exposure to the vapour.		Temperature of the animal previous to its exposure.	
		Minutes.	Cent.	Fahr.	Cent.	Fahr.	Cent.	Fahr.
1	1st. rabbit	39	38·7°	101·66°	42·4	108·32°	40·0	104
2	Do.	55	38·7	101·66	43	109·40	39·6	103·28
3	Do.	52	40·7	105·26	43·6	110·48	40·0	104
4	2d rabbit	55	38·7	101·66	42·9	109·22	39·6	103·28
5	Do.	75	38·7	101·66	42·7	108·86	40·0	104
6	Do.	55	40·7	105·26	43·1	109·58	39·7	103·46
7	Guinea pig	56	37·7	99·86	42·7	108·86	39	102·20
8	Do.	55	38·7	101·66	42·9	109·22	39	102·20
9	Do.	48·5	40·7	105·26	43·5	110·30	39·0	102·20
10	Do.	55	40·7	105·26	44·2	111·56	38·4	101·12
11	Pigeon	55	37·7	99·86	43·8	110·84	42·5	108·50
12	Do.	40	40·7	105·26	45	113	41·9	107·42
13	Do.	42	41·9	107·42	46·9	116·42	41·8	107·24
14	1st. frog	73	25·6	78·08	26	78·80		
15	2d. frog.	50	27·2	80·96	27·8	82·04		

I shall here subjoin some remarks with regard to the observations in this table. Remarks on them.

Beside the experiments, of which I have given the results, I made many others, on the accuracy of which I could not equally depend. Their results however were analogous to those in the table. Others made.

Whatever precautions I took to prevent it, there were always some variations in the temperature of the apparatus during the experiment. These variations did not in general exceed one degree [1·8° F]; but sometimes they amounted to 3° [5·4° F.], though for a very short period. The heat not quite constant.

When I employed the same animal in different experiments, I always suffered at least twenty-four hours to elapse between them. Animals.

The different thermometers I employed not being uniform in their motions, I examined these with care, and formed a particular scale of reduction for each, by means of which I reduced the several results of the different observations to one common scale. Though I have employed tenths of a degree in noting the results, I do not pretend to have been always thus exact in my observations; but I chose rather to express them thus, than to commit voluntary errours. The errours I have committed involuntarily however cannot at any time have exceeded a quarter of a degree. Thermometers.

The temperature of an animal previous to its introduction into the apparatus has frequently exhibited trifling differences, the cause of which I could not ascertain. Previous temperature.

It was not easy to ascertain the temperature of the frogs immediately on their being taken out of the box, and without its being influenced by the contact of the hands or the external air. To effect this, I tied the animal on a kind of cart made for the purpose, and placed a thermometer with a very small bulb, so as to remain in its mouth, or rather in its stomach. On opening the box, I had only to take out the cart quickly, and examine the degree indicated by the thermometer. Management of the frogs.

On looking at the table we perceive, that the temperature of the warm-blooded animals uniformly rose two or three degrees at least above the moist air, in which they were immersed; whence it is evident, that the faculty of The temperature of the animals above that of the medium.

producing cold was annihilated in them, and consequently that this faculty is essentially dependent on evaporation. It is true, that the heat, to which these animals were exposed, did not exceed their natural temperature by two degrees [3·6° F.]; and it might be supposed, that the faculty of producing cold would have been displayed by them at higher temperatures. But this objection will vanish, if we consider, that death would have been the necessary consequence of their exposure for a time of any continuance to a moist heat, greater than that to which I subjected them, and consequently that this faculty would have been extinguished in them. In fact, however low the heat they endured in these experiments may appear to have been, they were always more or less exhausted by it; and when it was greater, they appeared dying on my taking them out of the apparatus. The guinea pig, though very lively in the morning, died in the evening after experiment 10. I had likewise a rabbit and a pigeon that died after similar experiments, the results of which are not inserted in the table.

They could not have borne a higher heat long.

It killed some.

Why did the heat of the animals increase.

It may be asked perhaps, why the temperature of these animals did not merely rise to an equilibrium with the surrounding fluid, instead of exceeding it by some degrees. The answer to this question is very simple. The exercise of their functions not having been disturbed, the cause, whatever it is, that produces animal heat, must have continued to act on them, and occasion this rise of temperature. It is more difficult to conceive, why this rise was not greater; and why the same cause, which in low temperatures keeps animals at 20°, 40°, or even 80° [36°, 72°, or 144° F.] above that of the surrounding air, does not rise more than 3° or 4° [5·4° or 7·2° F.], when they are exposed to heat*. This question cannot be solved, till we have a satisfactory answer to another of great importance, that has been often debated: "what is the cause of animal heat?"

* Fresh experiments that I have made since this paper was read to the Institute, and which I shall soon make public, lead me to think that evaporation was not entirely suppressed in those I have here related: but these results, instead of invalidating what I have advanced, tend rather to confirm them.

a question

a question which, as may be seen from the facts I have adduced, is not essentially connected with that here discussed.

Remark on frogs.

In frogs, and I believe it would be the same with other cold-blooded animals, the difference between their temperature and that of the surrounding medium was always much less striking than in warm-blooded animals, as might naturally be expected. This however has afforded me an opportunity of making a remark somewhat curious, but requiring to be confirmed by repeated experiments, namely, that the proper heat of these animals, or the excess of their temperature over that of the surrounding medium, is as considerable when they are exposed to heat, as when they are exposed to cold. This would seem to indicate, that the cause of this heat is not the same as in warm-blooded animals.

Curious fact respecting them.

The production of cold in animals owing to evaporation,

From what has been said we may conclude, that the production of cold, manifested in animals exposed to a high degree of heat, is to be classed with those phenomena, the essential cause of which is physical. In this however we cannot overlook the influence of vital causes, which, as I have announced at the commencement of this paper, concur with the physical causes in the production of almost all the phenomena, that are the result of organization. In fact, the evaporation, that causes this production of cold, cannot take place, unless the surface of the body and of the pulmonary cells be kept constantly moist. And here the comparison of inorganic bodies, such as were employed in my experiments, ceases to be exact. The surfaces of these were moistened by transudation only. Those of animals are moistened by perspiration, a very complex phenomenon, necessarily depend on the action of the system of capillary vessels. In the former, the surface no sooner begins to dry, than it draws from the interior a new portion of moisture. In the latter, on the contrary, perspiration must acquire a fresh degree of activity, when the heat becomes more considerable; and this can take place only from an increased energy in the exhalant system, and perhaps even throughout the whole of the circulation. It is to be observed, that this increased activity of the perspiration, at least

in which vital causes concur.

least from the surface of the body, is more considerable, than is requisite to furnish the increased evaporation. Hence that sweat, which, in most cases, is nothing more than the excess of the fluid perspired above that carried off by evaporation.

Sweat.

General deduction.

I shall conclude this paper with the following proposition, which I think I may venture to advance as a necessary consequence of the observations contained in it. "The production of cold, manifested in animals exposed to a high degree of heat, is the result of the evaporation of the perspirable matter; which, in consequence of the increased action of the exhalant system, is so much the more considerable, in proportion as the external heat is greater. It is therefore at the same time the result both of physical and vital causes."

VII.

A new and expeditious Mode of Budding. By Thomas Andrew Knight, *Esq. F. R. S**.

Nurserymen apt to substitute one fruit for another.

PARKINSON, in his Paradisus Londoniensis, which was published in 1629, has observed, that the nurserymen of his days had been so long in the practice of substituting one variety of fruit for another, that the habit of doing so was almost become hereditary amongst them: were we to judge from the modern practice, in some public nurseries, we might suspect the possessors of them to be the offspring of intermarriages between the descendants of those alluded to by Parkinson. He has, however, mentioned his "very good friend, Master John Tradescant," and "Master John Miller," as exceptions; and similar exceptions are, I believe, to be found in modern days. It must however be admitted, that, wherever the character of the leaf does not expose the errour of the grafter, as in the different varieties of the peach and nectarine, mistakes will sometimes occur; and therefore a mode of changing the variety, or

Cause of mistake.

* Transactions of the Hort. Soc. vol. i, p. 194.

of

of introducing a branch of another variety, with great expedition, may possible be acceptable to many readers of the Horticultural Transactions.

Expeditious mode of changing a variety.

The luxuriant shoots of peach and nectarine trees are generally barren; but the lateral shoots emitted, in the same season, by them, are often productive of fruit, particularly if treated in the manner recommended by me in the Horticultural Transactions of 1808 *. In the experiments I have there described, the bearing wood was afforded by the natural buds of the luxuriant shoots; but I thought it probable, that such might as readily be afforded by the inserted buds of another variety, under appropriate management. I therefore, as early in the month of June, of the year 1808, as the luxuriant shoots of my peach trees were grown sufficiently firm to permit the operation, inserted buds of other varieties into them, employing two distinct ligatures to hold the buds in their places. One ligature was first placed above the bud inserted; and upon the transverse section through the bark: the other, which had no further office than that of securing the bud, was applied in the usual way. As soon as the buds (which never fail under the preceding circumstances) had attached themselves, the ligatures last applied were taken off: but the others were suffered to remain. The passage of the sap upwards was in consequence much obstructed, and the inserted buds began to vegetate strongly in July: and when these had afforded shoots about four inches long, the remaining ligatures were taken off, to permit the excess of sap to pass on; and the young shoots were nailed to the wall. Being there properly exposed to light, their wood ripened well, and afforded blossoms in the succeeding spring: this would, I do not doubt, have afforded fruit; but that, leaving my residence at Elton, for this place, I removed my trees; and the whole of their blossoms, in the last spring, proved, in consequence, equally abortive.

* Page 38: or Journ. vol. xviii, p. 196.

VIII. *Notice*

VIII.

Notice respecting Native Concrete Boracic Acid: By SMITHSON TENNANT, *Esq. F. R. S. &c. Communicated by* L. HORNER, *Esq. Sec. of the Geological Society**.

Boracid acid found but in few places.

THE boracic acid is not found, like the greater number of substances, in almost every country; but, as far as our present knowledge extends, appears confined to a few particular places. On this account, as well as the great utility of borax in various arts, the discovery of its existence in any new situation may deserve to be recorded.

Volcanic product from Lipari.

Some months ago Mr. Horner was so obliging as to show me a collection of volcanic productions from the Lipari Islands, presented to the Geological Society by Dr. Saunders. They consisted chiefly of sulphur, and of saline sublimations on the lava; but among these more common substances there were several pieces of a scaly shining appearance, resembling boracic acid. The largest of these had been cut of a rectangular shape, and was about seven or eight inches in length, and five or six in breadth, as if it had been taken from a considerable mass. On one side of most of the pieces was a crust of sulphur, and the scaly part itself was yellower than pure boracic acid. To ascertain if the scaly part was coloured by sulphur, I exposed it to heat in a glass tube, and, after the usual quantity of water had come over, there sublimed from it about a tenth of its weight of sulphur, and the remainder was pure boracic acid.

Native boracic acid mixed with a tenth of sulphur.

Another specimen.

Mr. Horner afterward informed me, that the late Dr. Menish of Chelmsford had presented to the Geological Society a specimen, which he had received, with some other volcanic productions, from Sicily, but which had been collected in the Lipari Islands; the box containing them being marked "Produzioni Volcaniche raccolte nelle Isole Eolie da Gius. Lazzari. Lipari." He found it to consist of boracic acid; and it perfectly resembled that I have just

* Trans. of the Geol. Soc. vol. i, p. 389.

described

described, having the same yellow colour from an admixture of sulphur, and a similar crust of this substance adhering to one side.

Any future traveller, visiting those countries, would do well to examine them with a view to this particular object. The boracic acid may be a more extensive volcanic product than has hitherto been imagined; for in the account given of its discovery some years ago, by Messrs. Hoefer and Mascagni, near Monte Rotondo, to the west of Sienna, we can have no doubt of its volcanic origin in those places, from the substances which are there described to accompany it. Probably a less rare volcanic product than might be supposed.

IX.

Notice respecting the Decomposition of Sulphate of Iron by Animal Matter: by W. H. PEPYS, *Esq. F. R. S. Treasurer of the Geological Society*.*

As the following circumstance, that took place in my laboratory, appears to throw considerable light on the mode whereby organic remains become penetrated by pyrites, it may not perhaps be foreign to the objects of the Geological Society; and, as such, I have taken the liberty of offering it to their attention. Mode in which organic matters are penetrated by pyrites.

I was engaged a few years ago in a course of experiments on hidrogen gas, which was procured in the usual method, by the solution of iron turnings in diluted sulphuric acid. The sulphate of iron hence resulting, to the amount of some quarts, was poured into a large earthen pitcher, and remained undisturbed and unnoticed for about a twelvemonth. At the end of this time, the vessel being wanted, I was about to throw away the liquor, when my attention was excited by an oily appearance on its surface, together with a yellowish powder, and a quantity of small hairs. A solution of sulphate of iron acted on by animal matter.

The powder, on examination, proved to be sulphur; and on pouring off carefully the supernatant liquor, there was Results.

* Trans. of the Geol. Soc. vol. i, p. 399.

was discovered at the bottom of the vessel a sediment consisting of the bones of several mice, of small grains of pyrites, of sulphur, of crystallized green sulphate of iron, and of black muddy oxide of iron.

Part of the salt deoxigenated by it.

These appearances may with much probability be attributed to the mutual action of the animal matter and the sulphate of iron, by which a portion of the metallic salt seems to have been entirely disoxigenated.

X.

Analyses of Minerals: by MARTIN HENRY KLAPROTH, *Ph. D. &c.*

Analyses by Klaproth.

As the fourth and fifth volumes of Klaproth's work have not appeared in English, and are not likely to be translated, the results of his analyses probably will not be unacceptable. Not having the work itself, they are taken from the *Journal de Physique*.

Electrum of Pliny.

Analysis of electrum.

This name is taken from Pliny, who says, book 33, chap. 4, § 23. "In all gold there is silver, in various proportions. Sometimes a tenth, sometimes a ninth, sometimes an eighth part. When there is a fifth part of silver, it is called electrum".

Gold ore of Schlagenberg.

In a gold ore from Schlagenberg in Siberia Klaproth found

Gold - - -	64
Silver - - -	36
	100

Silver ore from Peru.

From a species of silver ore, called in Peru *pacos*, some of which was brought over by von Humboldt, he obtained

Silver - - -	14
Brown oxide of iron -	71
Silex - - -	3·5
Sand - - -	1
Water - - -	8·5
	98

Conchoid

Conchoid muriated* silver from Peru. — Another.

Silver - - -	67·75
Oxigen - - -	32·25
	100

Native cinnabar from Japan. — Native cinnabar of Japan,

Mercury - - -	84·50
Sulphur - - -	14·75
	99·25

Native cinnabar from Neumarktel, in Carinthia, gave the same proportions. — and of Neumarktel.

Hepatic cinnabar from Idria. — Hepatic cinnabar of Idria.

Mercury - -	81·80
Sulphur - - -	13·75
Charcoal† - -	2·30
Silex - - -	0·65
Alumine - - -	0·55
Oxide of iron - -	0·20
Copper - - -	0·02
Water - - -	0·73
	100·

Red lamellar copper from Siberia. — Red lamellar copper.

Copper - - -	91
Oxigen - - -	9
	100

Kupferlazur (radiated mountain blue) from Silesia. — Radiated mountain blue.

Copper - - -	56
Oxigen - - -	14
Carbonic acid - -	24
Water - - -	6
	100

Malachite differs from this only in containing less carbonic acid, and more water. — Malachite.

* So in the Journ. de Physique. C.

† I do not know the word used by Klaproth: but as the French translator renders it *charbon*, not *carbon*, I have thought it best to employ charcoal. C.

Kupfergruen

Mountain green.

Kupfergruen (mountain green) or chrysocol from Siberia.

Copper - - -	40
Oxigen - - -	10
Carbonic acid - -	7
Silex - - -	26
Water - - -	17
	100

Vitreous copper ore.

Kupferglanzerz (vitreous copper ore).

Copper - - -	76·5
Iron - - -	0·5
Sulphur - - -	22
Loss - - -	1
	100

Gray copper ore from Freyberg.

Fahlerz (gray copper ore) from Freyberg.

Copper - - -	41
Silver - - -	0·4
Arsenic - - -	24·1
Iron - - -	22·5
Sulphur - - -	10
Loss - - -	2
	100

Another.

Fahlerz from Kroner mine, Freyberg.

Copper - - -	48
Silver - - -	0·5
Iron - - -	25·5
Arsenic - - -	14
Sulphur - - -	10
Loss - - -	2
	100

Another.

Fahlerz from Jonas mine, Freyberg.

Copper - - -	42·5
Silver - - -	0·9
Iron - - -	27·5
Antimony - - -	1·5
Arsenic - - -	15·6
Sulphur - - -	10
Loss - - -	2
	100

Crystallized

Graugultigerz from Kapnik,

Crystallized graugultigerz from Kapnik.

Copper	37·75
Antimony	22
Zinc	5
Iron	3·25
Sulphur	28
Silver } Oxide of manganese	0·25
Loss	3·75
	100

from Poratsch,

Graugultigerz in mass from Poratsch in Lower Hungary.

Copper	39
Antimony	19·50
Iron	7·50
Mercury	6·25
Sulphur	26
Loss	1·75
	100

from Annaberg,

Graugultigerz in mass from Annaberg.

Copper	40·25
Silver	0·30
Antimony	23
Iron	13·50
Sulphur	18·50
Arsenic	0·75
Loss	3·70
	100

from Zilla,

Crystallized graugultigerz from Zilla, in Clausthall.

Copper	37·5
Silver	3
Antimony	29
Iron	6·5
Sulphur	21·5
Loss	2·5
	100

Graugultigerz

from St. Wenzel,

Graugultigerz from St. Wenzel, at Wolfach.

Coppper	- - -	25·50
Silver	- - -	13·25
Antimony	- -	27
Iron	- - -	7
Sulphur	- - -	25·50
Loss	- - -	1·75
		100

and from Peru.

Graugultigerz in mass from Peru, brought over by Humboldt, and taken from the vein called Purgatorio.

Silver	- - -	10·25
Copper	- - -	27
Antimony	- -	23·50
Iron	- - -	7
Lead	- - -	1·75
Sulphur	- - -	27·75
Loss	- - -	2·75
		100

Triple sulphuret of lead from Segen,

Ore of Antimony and lead from Segen, in Clausthal.

Lead	- - -	42·50
Antimony	- -	19·75
Copper	- - -	11·75
Iron	- - -	5
Sulphur	- - -	18
Loss	- - -	3
		100

and from Andreaskreuz.

Ore of antimony and lead from Andreaskreuz, St. Andreasberg.

Lead	- - -	34·50
Silver	- - -	2·25
Copper	- - -	16·25
Antimony	- -	16
Iron	- - -	13·75
Sulphur	- - -	13·50
Silex	- - -	2·50
Loss	- - -	1·25
		100

(To be continued).

SCIENTIFIC

SCIENTIFIC NEWS.

Wernerian Society.

AT the meeting of this society on the 22d of February a communication from the Rev. Mr. Fleming of Flisk, was read, describing the mineralogical appearances, which occur on the north bank of the Frith of Tay, from Dundee up to Kingoodie quarry. The rocks are claystone, claystone-porphyry, felspar-porphyry, greenstone, sandstone and amygdaloid. The sandstone occurs in basin-shaped cavities in the porphyry, and contains subordinate beds of greenstone; but he deferred giving any decided opinion concerning the geognostic relations of these rocks, till he should examine the south shore of the Frith of Tay.

Mineralogy of the Frith of Tay,

At the same meeting, the secretary read a communication from Mr. Macgregor, surgeon to the 25th regiment, giving an account of the mineralogy of the country around the town of Lanark, particularly at the celebrated falls of Cora Lin and Stonebyres. Near the former, porphyry-slate and felspar-porphyry occur. At the latter, the waters are poured over beds of fine grained sandstone, which, in descending, gradually becomes coarser in texture, till it passes into a conglomerate, consisting of masses of quartz, jasper, splintery hornstone, flinty-slate, and clay-slate. Near Nethan bridge, the traces of a coal deposition and a portion of a coal-field make their appearance; many alternating beds of sandstone, bituminous shale, and clay ironstone occurring along with thin beds of slate coal and cannel coal. Mr. Macgregor stated it to be his opinion, that the sandstone exposed on the banks of the Clyde and of the Mouse river near Lanark, belongs to one and the same formation; that the Mouse has gradually scooped out the present channel, in the same way as the Clyde is supposed to have done; and that there are here no marks of any violent convulsion of nature, as some have imagined.

and of the country round Lanark.

An extract of a letter from Lieutenant Huey of the 73rd regiment was also read, mentioning the circumstance of a large marine animal, supposed to be about thirty feet long, and shaped like a snake, having been observed from a ship in lat. 38° 13 S. and long. 5° E.

Large sea snake.

At

British sponges. At the meeting of this society on the 7th of March, the secretary read an "Essay on sponges, with descriptions of all the species, that have been discovered on the coast of Great Britain", by George Montagu, Esq. of Devonshire.

Nature of sponge. From Mr. Montagu's researches as to the constitution of sponges, it appears, that no polypi or vermes of any kind are to be discerned in their cells or pores: they are, however, decidedly of an animal nature; but they possess vitality without perceptible action or motion. Mr. Montagu has divided the genus spongia into five families: viz. Branched, digitated, tubular, compact, and orbicular. Only fourteen species were previously known to be British: Mr. Montagu, in this communication, described no fewer than thirty-nine. A considerable number of the species are quite new, or have now for the first time been distinguished and formed by that indefatigable naturalist.

Natural method in botany. At the same meeting, Dr. Yule read a memoir on the natural method in botany, in which he defended the existence of the series of natural affinity in plants against the objections of professor Willdenow and Dr. Smith, founded on the want of regularity in the series, &c. He contended, that the illustrious author of the artificial system never intended, that it should supersede, but on the contrary, that it should lay the foundation of the Natural Classes, "quas plana genera nondum detecta revelabunt:" and that with this view he uniformly inculcated the study of natural genera, in conformity with his great maxim, "Omne genus naturale."

Literary and philosophical society of Liverpool. A literary and philosophical society has been lately instituted in Liverpool. Its object is to collect information in all branches of literature and science, which is laid before the society in the form of Essays, or Papers. The number of members already amounts to near sixty, and their meetings are held monthly from October to May inclusive. The communications and attendance are entirely voluntary. Officers; the Rev. Theophilus Houlbrooke, President: Rev. Joseph Smith, Dr. Bostock, and John Theodore Koster, Esq., annual Vice Presidents: and Dr. Tho. Stewart Traill, Secretary, to whom communications are to be addressed.

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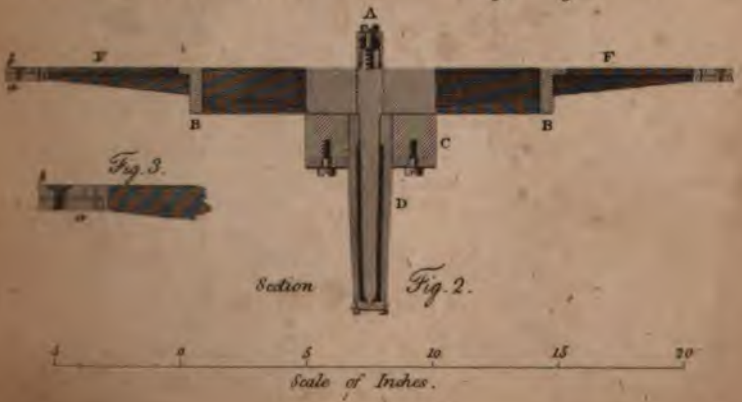



Mechanism of Flowers.
Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.

Mechanism of Flowers.

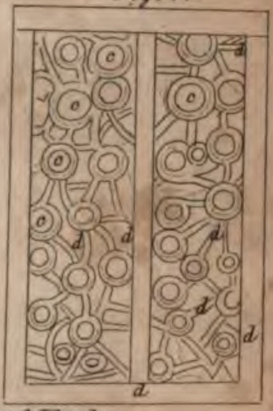
Fig. 2.

Fig. 1.

Fig. 5.

Fig. 3.

Fig. 4.



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grains 4 3 2 1 0

Mr. Reid's Compensation Pendulum.

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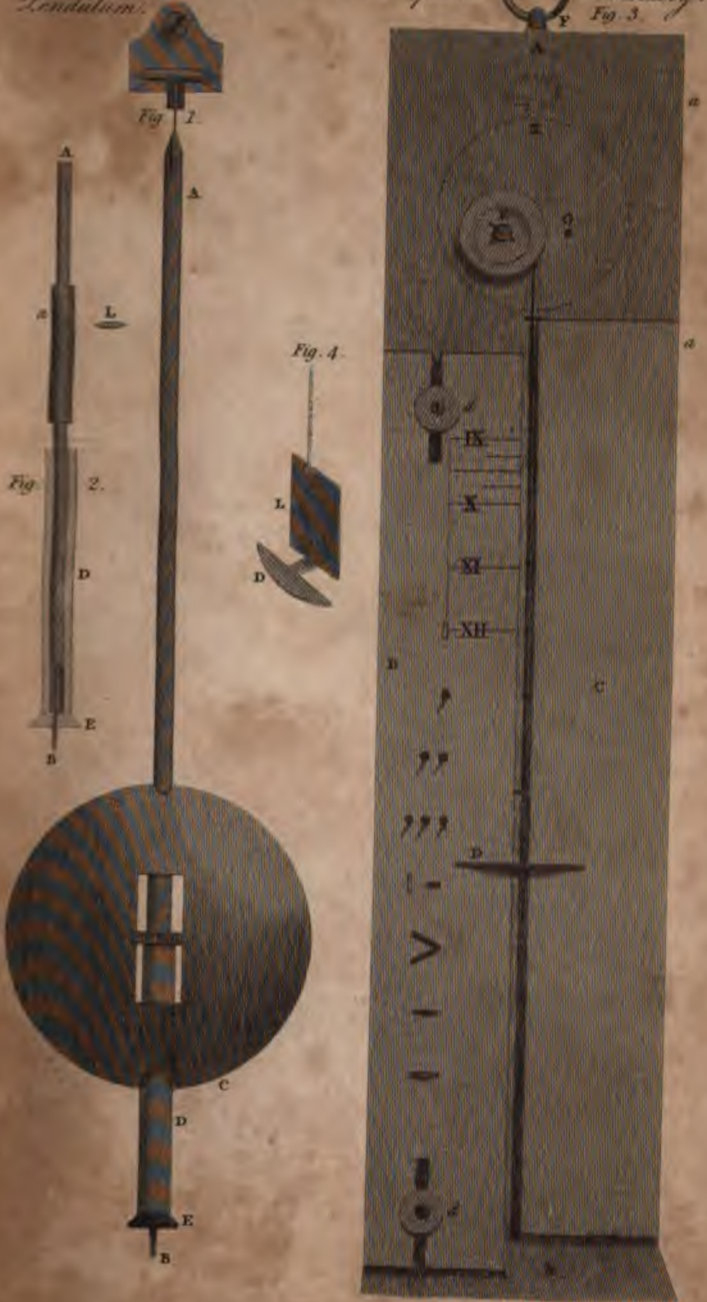


Nicholson's Philos. Journal, Vol. XXXI Pl. VII p. 24

Dissections of Aquatic Plants

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.

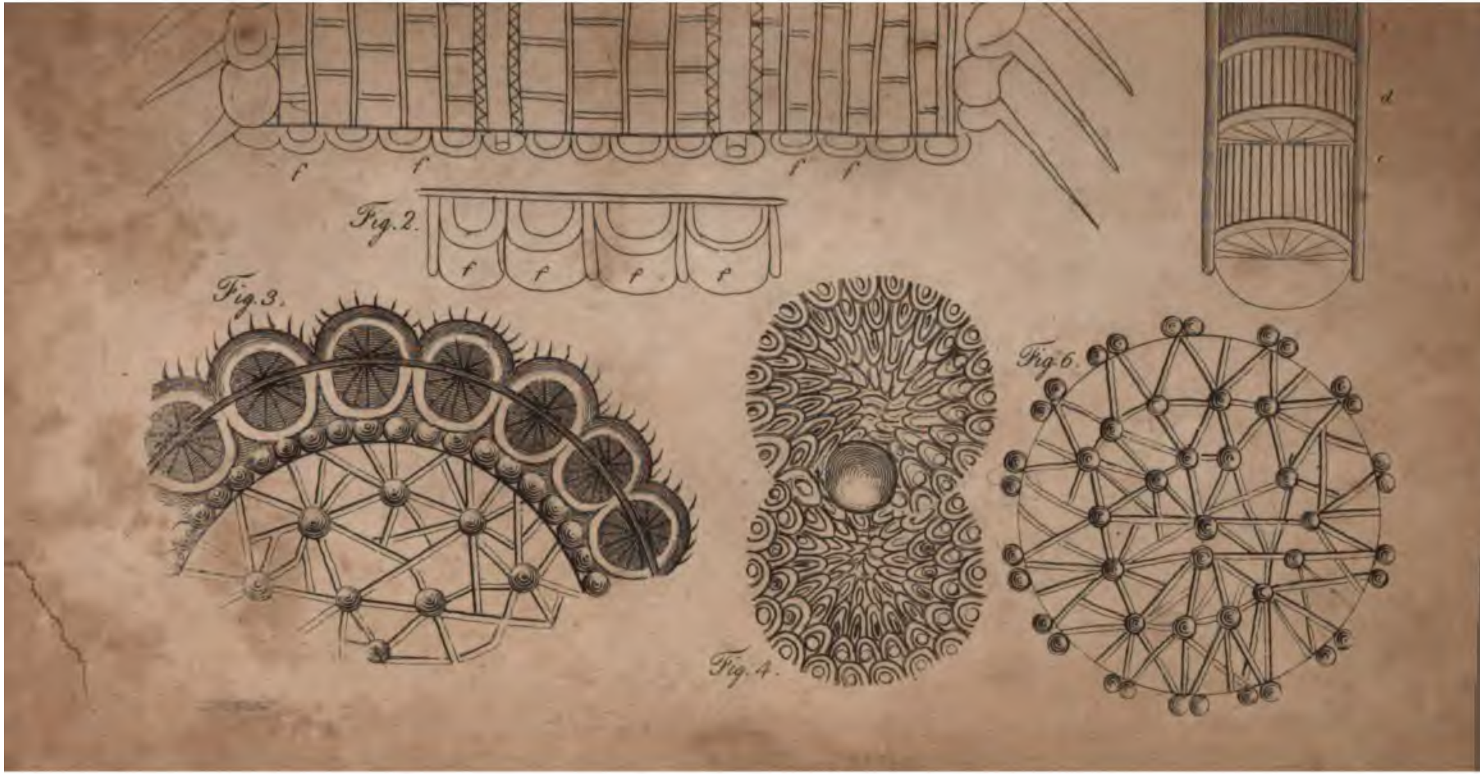
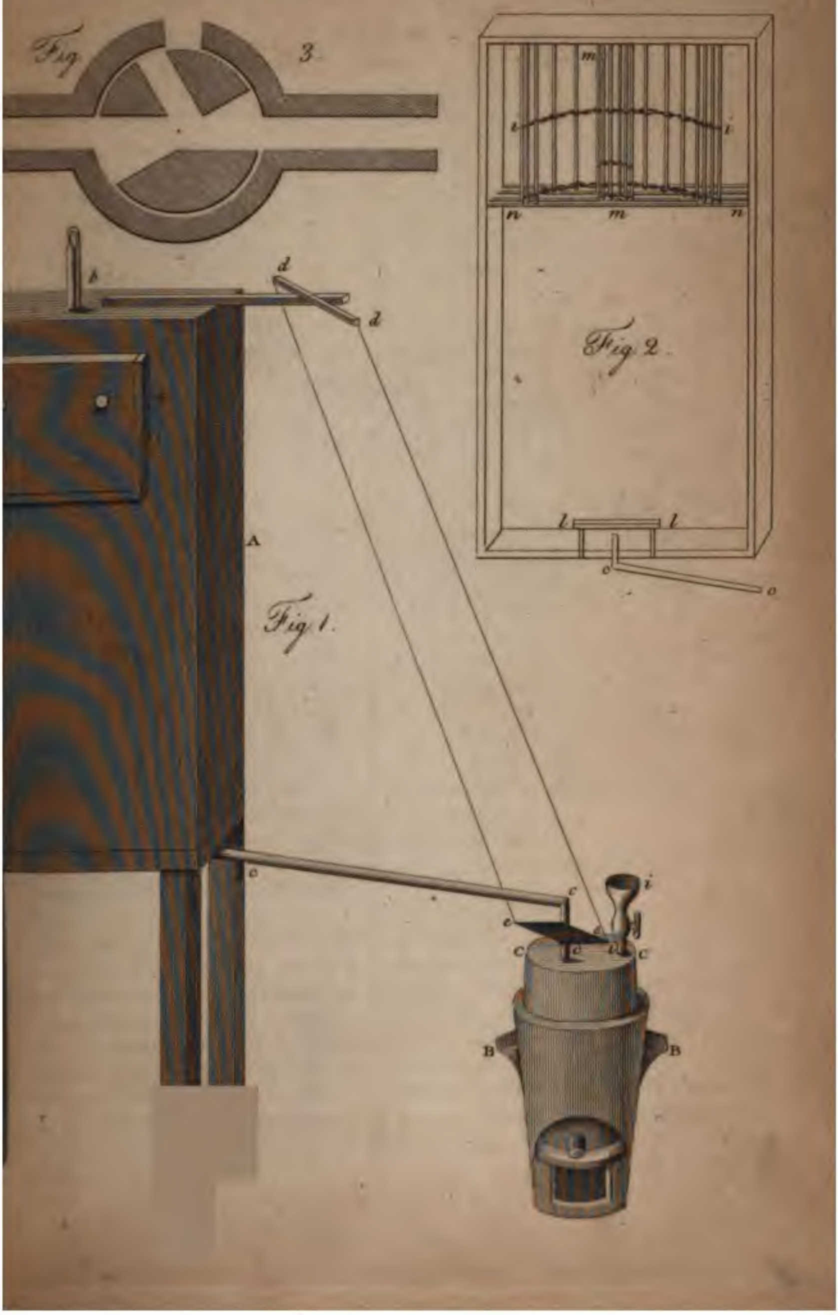


Fig. 2. Fig. 3. Fig. 4. Fig. 6.

Delaroche's Apparatus for exposing Animals to heat.



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END OF THE THIRTY-FIRST VOLUME.

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[redacted]

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PREFACE.

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Of Foreign Works, M. J. Fabbroni, Cor. Mem. of the French Institute; M. D'Arcet; M. Schrader; Mr. Laugier; D. J. Carradori; M. Vauquelin; Dr. Wienhold; M. Bucholz; Prof. Curaudau; Prof. Klaproth; M. Sonnini; M. Thenard; M Braun; A. S. Duportal, M. D.; M. T. Pelletier; M. Chevreul; M. Robinet; Prof. J. B. Vitalis; M. Leschenault; M. J. C. Delamétherie; M. H. Flaugergues; and Prof. H. Braconnot.

And of British Memoirs abridged or extracted, A. Marcet, M. D. F. R. S.; &c. W. T. Brande, F. R. S.; H. W. Way, Esq.; Mr. E. Smith; F. Fortune, Esq. Mr. P. Sleavin; N. Nugent, M. D. Hon. Mem. of the Geol. Soc.; J. Davy, Esq.; S. Tillard, Esq. Capt. R. N.; J. Stephens, Esq.; W. Jones, Esq.; Mr. C. Waistell; Mr. T. Dickson, Mr. J. Fuller; C. H. Parry, M. D. F. R. S.; T. A. Knight, Esq. F. R. S. Prof. Hort. S. &c.; and Mr. J. Maher, F. H. S.

The Engravings consist of 1. Dissections of Plants of the Class Cryptogamia, delineated from Nature, by Mrs. A. Ibbetson, in two Plates. 2. Mr. E. Lydiatt's Smicrologometer, for ascertaining the Tenacity of Metals, and Strength of Threads of Silk, Cotton, Linen, &c. 3. A Dissection of a Flower, in a 4to. Plate: and 4. A Branch of Laburnum, with a Section considerably magnified; all delineated from Nature by Mrs. Agnes Ibbetson. 5. Closure and draining Bricks, by J. Stephens, Esq. 6. Method of constructing a temporary Rick for securing Corn in wet Weather, by W. Jones, Esq. 7. An improved Dibble for planting Acorns in Bushes, by Mr. C. Waistell. 8. A Potato, with the Method of taking Sets from it for preventing the Curl, by Mr. T. Dickson. 9. Diagram illustrative of Electric Attractions and Repulsions, by J. C. Delamétherie. 10. Diagram illustrating the Law of Evaporation, by Honoré Flaugergues. 11. Different Modes of constructing the Breech of a Gun, so as to make it throw the Shot close, or scattering, by a Correspondent. 12. An improved Scarificator, by Mr. John Fuller.

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AUGUST.

AUGUST, 1812.

SUPPLEMENT

SUPPLEMENT TO VOL. XXXII.

A
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OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

MAY, 1812.

ARTICLE I.

On the Fructification of the Plants of the Class Cryptogamia. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

IN my last letter I showed the dissection of fresh-water plants, endeavouring in a particular manner to mark the effect produced in different vegetables by the more or less water which surrounded them in their growing state; and proving, that those large divided air vessels are to be found in fresh-water plants alone; the vessels decreasing as the ditch, in which they were in the habit of growing, approached more to boggy or wet ground instead of water. This is truly exemplified in what I have called the half-water plants: there are however a few exceptions to this rule; and, since I last wrote, one has occurred to me in the *arum*, which, though long removed to tolerably high ground, still retains its immense air vessels. But in comparing fresh-water plants with marine plants, the alteration and transition is excessive. Instead of large bladders of air, circular wood vessels

Difference of boggy and water plants.

Marine plants.

vessels, and the strongly marked vital line, I find an extremely compressed formation, so delicate and fine, that it is very difficult to comprehend its uses and capabilities.

Difficulties of the study of the cryptogamiæ.

But before I enter on the subject of the cryptogamian plants, I must say a few words in vindication of an undertaking, that may appear to many (considering the number of learned men that have written on the subject) so little necessary. Linnæus might be said to select all the difficulties of botany, and unite them in one class. Yet though the various genera differ so much from each other, they are certainly most properly arranged, since they carry strong marks of internal resemblance; of which, I doubt not, that great master had a perfect knowledge. The very difficulties of the study appear to have constituted part of the charm, which has tempted such numbers to seek for, and try to understand this class of plants. Hence we find so many masters, who have dedicated their whole lives to the perfecting the knowledge of one single genus of the cryptogamian plants. This being the case, will it not be construed into extreme vanity in me, to select such a subject? Yet the plan I have formed cannot be complete without it; and there is certainly one part, that has not yet been touched: neither Gmelin, Dillenius, nor Stackhouse has dissected the interior of these plants. No master has proceeded farther than selecting and describing them, and giving their habitats: all which is so admirably shown in that incomparable work, the joint labours of Dr. Smith and Mr. Sowerby. This part therefore, "the dissection of the interior of plants," I may venture to appropriate; and should I, in the review I mean to take of the whole class, contradict the assertions of any of the great men I have before mentioned; it will, I hope, be considered, that I only venture to do it from possessing more powerful means of magnifying than they did, rendering the objects clear and luminous; which constant study has taught me the means of doing with effect.

Few masters have dissected cryptogamian plants.

Importance of the line of life in indentifying plants.

If the vital part of a plant was productive of no other consequence than that of marking its existence, I should not so continually have pressed it on the notice and attention of the public: but it is the centre from which every other line must take its rise, it is the point which must certify

tify the identity of every other part. Thus, by tracing the vital line, the seed, the bud, the flower, the radicle, are all ascertained and proved. The interior vessel of the pistil is formed by this line alone, which, being a cylinder, conveys the mixed juices to the seeds. I have shown this before in all other plants, but it is to the cryptogamia I trust for completing the proof of all I have before advanced on this subject. Its admirable conformity in the direction of its vessels; its agreeing in all points of the fructification, not only with each genus of this class, but with all others; establishes (in my opinion) the truth of both in an eminent degree. I have said also, that the wood conveyed the peculiar juice for the formation of the pollen: and I trust the 24th class will exemplify the truth of the fact; for in the interior of these plants, and by the direction of those two vessels, will botanists be alone able to discriminate and identify the stamen and pistil of these diminutive vegetables. To prove this I shall first show the formation of marine plants; and then endeavour to explain the fructification of the cryptogamia in general, and mark, by the direction of the vessels, which is the stamen and pistil of each plant. Stamen and pistil traced by lines.

Though the marine plants, (such as the fuci and ulvæ), have the appearance of stems, yet in the greatest part of these plants it is appearance only. When subjected to the strongest magnifiers, placing a thin cutting of each in a solar microscope, they present exactly the same picture, except that the stalk is thicker and more compressed than that which is properly named leaves. As the sea weeds are almost without vessels, (at least have only two or three in a large surface) they have of course no liquid of the nature of sap to diffuse into different parts of the plant. This is proved by one part drying and dying, though the adjoining part is immersed in water; the former not benefitting by this, as it has no vessels, that can convey the moisture; which, I suppose, is given merely by pores at the surface, and passes not from one bleb to the other. Interior formation of the fuci.

The fuci might be properly divided into thin and thick fuci: The 1st, as the dulse, the palmatus, coccineus, and all of this kind, consists of that transparent and almost invisible skin doubled; which, in all common leaves, makes a part Division of the fuci. Structure of the thin fuci.

part of the cuticle of each side. But, what is most extraordinary, this skin, instead of being without, is in the interior; and, if you lay the dulse (or any other of this kind) on a glass, and scrape it very carefully on both sides with a knife, you will find all the exterior rubbed off, and nothing will remain but the almost invisible skin. This roughness I take to be the bark, it is most regularly placed in diamonds, (see Plate I, fig. 1, dulse unscraped); and answers well to the same matter, either within or without the transparent skin, in almost all the cryptogamian plants of every different genus. We trace it in the roughness at the exterior of the lichen, under the clear skin in the thick fuci, and so on to most of the class. But in the thicker fuci the transparent skin is on the exterior; and when it is taken off, and also the thin rough bark, the consistence of the matter underneath differs greatly from that of the thin fucus. It is so glutinous, so capable of distention, that, if drawn out or pressed, after being laid in fresh water, it may be reduced to what appears its original formation; that is into cylinders or strings, formed as at fig. 2. They cannot properly be called vessels, for they certainly appear not to convey any liquid; but to be a glutinous mass, in this shape. On examination of all the different fuci I could procure, I could find only these two vessels in each plant: 1st. The line of life which passes to the pistil, and afterward ties the seeds together: 2d. The wood vessels, which run directly to the male, and convey not only its peculiar juice, but the spiral wire that produces its motion. To make this plain, to enable any person to discover immediately both the vessels, and the stamen and pistil, I shall give this easy rule: When the line of life appears in the interior of a plant alone, and no wood vessels are found, it is certain, that the male is in a different plant. When both line of life, and wood vessels, are found joined together, you may be sure to find the fructification in the same flower. And when both vessels are found, but separate, it is always a sign, that the stamen and pistil are in different parts of the same plant. This law holds good in all the cryptogamian plants, nor have I ever found it vary.

Formation of the interior of the thick fuci.

Only two vessels in the fuci.

Rules for finding the vessels, and the stamen and pistil.

Mistake corrected.

I must now apologize for a mistake I have made in my former

former letter, in saying, that the spiral wire was found only in the conferva of all this class of plants. But I had so often sought it in all the sea weeds, and in the lichens, without discovering the smallest traces of it; that I felt convinced it was not there. As it is found in the male plant only, few would undertake the labour that is necessary to find it. In the mosses however it abounds, and in the woody part of the lichens also; and particularly distinguishes the male plant, whether single or joined to the female, by its never-ceasing motion. So violent is it often, that it is with great difficulty that it can be confined sufficiently for inspection, especially when first taken from within the flower. This alone makes a very distinguishing mark of the male in all the cryptogamia, for the female is quite inert. When its size has permitted me to take out the spiral wire, it leaves the rest of the plant perfectly quiet. I have therefore in various cases absolutely ascertained, that it is this alone which is the cause of motion in all plants.

Motion the sign which distinguishes the stamen.

That the fructification should have been continually mistaken by those, who had no other rule but mere guess to which they could apply for the discovery, cannot appear astonishing to any one; since, not knowing the interior formation, they could neither appeal to its analogy, with respect to other plants; nor to any means except the appearance and figure. But, as I have long been accustomed to be led up to the female by a peculiar line; I sought this in all the cryptogamia, and directly found it. It was not indeed quite so easy to discover in the male plant; but remembering, that the wood in all other plants formed the stamen; and that I had every reason to be convinced, as there was a peculiar juice for the formation of the pollen, there must be some vessel to convey this: this idea excited my diligence in seeking it, and I soon succeeded; and not only found the wood vessel meandering from male to male, but discovered, that in this class the spiral vessels always accompanied it as in every other plant. It is of extreme consequence to trace these lines in the cryptogamiæ; since without them it is impossible, that any person can be assured, that the male and female, if separate, belong to the same plant; whereas the running of the wood vessels from part to part will

Why the fructification has been mistaken.

Search of the line leading to the male.

will quickly ascertain, whether it is the original or a parasite plant: if no wood vessel leads to it, it should be condemned at once.

Fructification of the fuci.

I shall now turn to the fructification of the cryptogamiæ; beginning with the sea weeds, but leaving out the conferva; which of itself would nearly occupy a letter. The fructification of the fuci in general is exemplified in a specimen of the fucus serratus; which I shall just describe. A jellylike mass, with seeds bearing granules, and external papillæ. Though the apparent anomaly, that prevails in the fructification of the fucus genus, is acknowledged by all, yet this variety is more in appearance, than in reality, as I shall prove in a future letter. Whatever may be the difference in the formation of the marine plants, and in their means of receiving nourishment; in all the general lines of their fructification they differ not from all other plants. The line of life composes the female plant invariably, and is always to be known by its direction; and the seeds are tied by the same line. Hence it is easy to discover it; since, wherever a branch is going to shoot (in the thick fuci especially) if we seek the line from which the bud proceeds, it will directly point out the line of life, or vital mark. The wood vessels are always to be traced to the male plant, whether carrying sap or no sap: for these are those peculiar juices already mentioned, of a more oily nature and wholly destined to the formation of the pollen. Fig. 3 is the tubercle of the fucus serratus; CC is the line of life leading to it: fig. 4 is a circle under the tubercle, which has rays proceeding from it, to which the seeds are always attached, and to which, let them appear ever so much scattered, they are invariably fixed. As to the male plant, it is certainly the pencilled vesicles on the frond as it is also in the vesiculosus and many others. When much magnified, they are very curious; in the first it is pitcher shaped, from out of which tubercle the powder proceeds. In the vesiculosus it is a sort of ring, in which the powder is formed, and worked into the hairs. In both the wood vessels meander from male to male, and the hairs (if prevented sticking on the frond) move much when breathed on, and when shaking out the powder from its filaments. See fig. 5, *ee* male, F the wood

Fucus serratus described.

General analogy between the marine and other plants.

Vesicles the male of the fuci.

wood vessel leading to it. The stamens are said to be permanent; but this is certainly not the case, since it is only once a year the powder is found in the hairs: but as the old ones remain a long time after they have performed their office, before they decay and fall off; it gives them the appearance of perpetuity.

The Ulvæ.

Though I at first intended to give a marine plant, yet being so thoroughly acquainted with the ulva crassata; I preferred showing its dissection. It is formed of a membranaceous frond, with minute thick set tufts of branched filaments jointed, and beaded; the female being the ball; and the top, which is perforated, constituting the pistil, (see fig. 8). Under the tufts, G, fig. 7, the seeds are imbedded in regular order, each holding by the line of life; see GG the pointed filament, which proceeds from the pistil, and the wood vessels of which run up round it, and serve as a cuticle to it; showing themselves also on each side of the capsule and its stem as seen at HH, fig. 8. When the plant is first taken out of the water, and gently dried, if its pollen is ripe, and the hairs stick not on the frond; when breathed on they move more than the males of the fuci. I have seen them rising and falling with a constant succession of motions, which gave to the plant an appearance of life difficult to describe; but if too wet, or too dry, they move not. I found much of this ulva in a pond at Bellevue, near Exeter. The ulva pruniformis much agrees with this; there are certainly two sorts; one resembling in its fructification the lemna, and the other the crassula; but, as I got it twice only, and then rather in a dry, decayed state, I was fearful of making some mistake, if I should attempt to review it.

Fructification of the ulvæ.

The Musci.

It is very painful to me, to be obliged to contradict those, whose superiority I so gratefully acknowledge; as every botanist must the uncommon labours of a Dillenius, a Michelli, or a Gmelin: yet I cannot but differ from them respecting the fructification, which I would thus describe. The

Fructification of mosses.

The flower of the moss standing on a long stalk, and having its male and female in the same flower, being a capsule on a peduncle, sheathed at the base, with its seed vessel in the interior, the pistil standing up in the middle; the veil investing the fringes, which are truly the male part of the plant, and keeping them close, till the powder of the pollen is ripe, then both veil and lid fall off, the fringe spreads, and as soon as the drop appears on the pointal, the inner fringe draws over it. Then by breathing on it (when under a strong magnifier) any person can convince themselves, that these hairs are the males, since they throw out the pollen from every spray, till the top is covered with its powder. But as the inner fringe stands up in a pinnacle, the powder generally falls under on the stigma, by which means it is not so conspicuous, and is soon dissolved by the liquid of the pistil, and thence carried to impregnate the seeds. The outer fringe has from 4 to 32 teeth, which are either reflected, straight or twisted, triangular, spear shaped, blunt, or sharp; while the inner fringe is much finer, either closely adhering to the outward, or joined to it by threads from its inner sides; but which ever way it is formed, it has powder, which works out from the interior of the fringes. Many have between the hairs little balls on foot stalks, out of which proceeds the powder; others a sort of division up the hairs, which, when moved, gives out the dust, so that the inner fringe always appears variously jagged. Nature seems to have formed the lid to keep the fringe together, and prevent the hairs throwing out the powder, ere the seeds are ready to receive impregnation, or the liquid of the pointal to dissolve the pollen. It is these beautiful provisions of nature, that should be so closely watched. Who can behold all the exquisite contrivance displayed in the formation of these fringes, and not be convinced, that they were intended for some important purpose? Never is such perfect mechanism seen without it is designed to produce some great effect. The sight directly excites my mind to discover the use; nor do I allow myself to pass on to another subject, till I have studied hard to find out the cause.

Fringes the male part of the plant.

Description of the fructification.

The seeds are numerous and spherical, and all tied together by a line, which is the line of life. The wood vessels constantly

constantly lead up to the capsule, in which they form stripes. That which used to be called the male is a cryptogamian plant, found in all these diminutive vegetables, and taken generally for the stamen of the mosses, filices, lichens, and others: for, as it grows always, and has the appearance of powder, the mistake was very natural to those who knew no law, by which a parasite plant could be distinguished from the identical plant on examination. Fig. 9 is the capsule; I, its interior: fig. 10, is a more highly magnified view of the outer fringe, K, and the inner fringe, L: figs. 11 and 12 are the pitcher-shaped leaves. Most mosses, when they first shoot, require much water; and there being a quantity of spiral wire in the leaves, they easily draw into this shape, and for some time retain water in each leaf by the contraction the moisture occasions. That it is the spiral wire, that passes up the capsule in the wood vessels, is plainly shown in the stripe that accompanies this part, and is more strongly evinced in the figure of the tortula subulata. When the upper case stops some way below the seed vessel, the stripe leaves the outer case and runs up the under, in the shape of a corkscrew, to form both fringes. See Sowerby's admirable print, which is very exact. Vol. 16, p. 1101. Both fringes move, and both must concur in the office of the male, since the spiral is worked to and fro from the outward to the inner vessel repeatedly; and is seen in the microscope to contract and dilate at the bottom of the capsule, as I have marked at N N, fig. 9. In the polytricha, that which is supposed to be the male plant has certainly not only stamens, but a pistil, and is of itself a completely distinct plant; the middle of which opens, and shows the pointal, while the teeth around unclose at the edges, and discover the pollen. In all that I am acquainted with this is the case, but I know only four; it is not often I could find the plant called the male, and then they were perfectly divided, having their own stem and root.

As to the mosses that have no apparent fringes, there may be some having the male flower in a different plant: but, if I may be allowed to say what I have often experienced in many cases, when the fringes are not to be found in the usual place, I seek it in the lid or veil, where I seldom miss finding

Some mosses may have the male in a different plant.

finding it. They are so delicate, that the smallest touch breaks them. I do not however deny, that there may be some gymnostoma thus unsupplied; nature possesses so much variety of form, but then it is not generally shown in these points: there is a strict conformity in all that concerns the fructification of plants, that teaches us to expect a change less in these matters than in any other parts. Besides, they might be bent or broken. I think I have dissected the g. viridissimum, and if it was the plant (and it concurred in every other point) it has a very narrow rim of inflected teeth, which grew dark as the powder ripened. The polytrichum commune is very curiously formed at the bottom of the capsule; the manner in which the spiral is laced displays a mechanism most wonderful; if it was possible to understand the whole management.

The Filices.

Fructification of the ferns.

The general structure of the fructification of the ferns is as follows. The scale or calyx is not often found. It springs out of the leaf, opening on one side, and is different from the cover. The wood vessel and line of life, forming together, run up to each set of flowers, which are dispersed in parallel lines oblique to the midrib, commonly in one row on each side of it, but sometimes the row is double. Under the cover, usually supported on little foot stalks, are the flowers, encompassed by an elastic ring, which is really the male part of the plant. When the seeds are ripe, the impregnating cord springs and moves with every change of temperature, till it has shaken out all the powder to be found in it. The capsules then burst, the seeds disperse from the force of the confined membrane within the seed vessel, which, having the seeds fastened to it, and being coiled up in a manner adverse to its form, (as the spiral wire within it grows stronger) it struggles to get free, which it does at last by bursting the capsule, and throwing off the seeds to a distance; in the same manner as it does in the spirting cucumber, and in many other plants of that kind. That the elastic line that covers the apparent basket is really the male part of the plant, is easily proved. Let any one place the cyathea fragilis under a strong magnifier about

the

the time the male powder is ripe. After observing the capsules to be covered on one side with a white shining mixture, they will soon see this turn to a pale green, from the powder which falls on it from the handle or elastic ring. The manner in which the pollen is given out is as curious as any part; for the ring contracts and dilates alternately, till it has yielded all its dust. Nor is there the least fear of taking the pollen for the seeds, the one being brown, the other almost white. In the month of September this mechanism is very plain in the asplenium scolopendrum; I have seen the male so difficult to confine from the eternal motion of the cord, that without a pair of pincers it was impossible to fasten it within the field of the microscope. Sometimes the fructification of this powder is in spikes, and then the flowers are contained within a case, as in the equisetum sylvaticum. There the male and female have been perfectly guessed; the capsule, which holds the seeds, being the pistil; while the agitated part attached to it is the stamen, and which may really be said to fly from the glass. I have seen them, when first thrown on the paper, move about like a worm, and if a drop of water is placed near them, the filaments gather round the capsule, as if to defend it; beating the anthers against it, till it is completely covered with powder. It has 4 filaments to each female.

Great agitation of the male plant.

Thus then we may lay down three rules for discovering the male plant: 1st, the leading up of the wood vessel to the part either with or without the line of life, as the male is or is not joined to the pistil. 2d, the constant motion of the filaments and anthers, when giving out their powder; which agitation belongs only to the male, for the female is perfectly inert. 3d, the stamens being almost constantly in the shape of hairs, which will lead a student at once to examine every thing in the cryptogamiæ that bears this appearance.

Rules for discovering the male.

The fructification of the filices is seen in Pl. II. Fig. 1, *oo*, the joint wood vessel and line of life leading up to the fructification, in the leaf of the scolopendrum vulgare: fig. 2, P, the capsule with the pointal; Q, the elastic line or stamen: fig. 3, the seeds tied by the line of life. The fructification of the equisetum sylvaticum, palustre, and arvensis,

arvensis, for they exactly resemble each other: fig. 9 the target carrying the flowers: fig. 10, an interior view of the same cut through the middle: fig. 11, the pistil and capsule bearing its seed, with 4 stamens attached to it by their filaments, one of which is shown still more magnified at fig. 12. I was proceeding to the agarics, but my letter already appears so long, that I shall leave these for my next communication, and join to them the lichens, jungermanniæ, and marchantiæ.

I am, sir,
Your obliged servant,
AGNES IBBETSON.

Cowley Cot.
March the 8th, 1812.

Supposed males of certain mosses.

P. S. I think it right, however, to add the three males of the mosses, which I have found, dissected, and exposed to very great magnifying powers. See fig. 7, which, like a number of others, proved merely a collection of leaves: fig. 8, which showed a sort of pistil in the middle concealed and covered by the stamen: and figs. 5 and 6, which appeared the male of a polytrichum, but were certainly a complete flower with both stamen and pistil. The perfection and exactness of Mr. Sowerby's drawings no one would venture to contradict, and I mean not in any manner to do so, I have too many opportunities to admire the perfect likeness of each object. All that I would wish to suggest is, that the plants taken for males in the mosses are plants of the same genus, and having both male and female, which by dissection may be found. To prove this, there are many arguments, most strong and powerful. That nature should have formed all this beautiful apparatus for nothing; that these exact and regular fringes, thus exquisitely formed, should be made to bend over the seed vessel at a certain time, and rub out a powder: that the veil should remain, without any reason, a stipulated time, then quit it, for as little apparent cause; is not like her general arrangements. But, on the contrary, that nature should have placed all this spiral wire in the fringes, that its motion, might rub out the pollen from the teeth: that the veil should remain on, to keep the males from moving, till the seed

seed is fit to receive impregnation; that it should then fall off, and the fringe bend over the pointal, to mix the pollen with the juice of the pistil; and, to prevent the powder of the stamen from being lost in the seeds, that a thick curtain should be drawn between, to give time for the melting of the powder in the sweet juices of the pistil; all this is exactly conformable to the process in every other flower, and analogous to the proceedings of every other plant. But this is not all: that in all the rest of the cryptogamiæ the males should be distinguished for excessive motion, and yet in the mosses alone be different, is not to be credited. Besides, when the supposed male flower is found; it is often not one to ten thousand females; and considering, that much powder must be lost in attaining the pistil, nature would have provided a quantity, as it does in every other case I am acquainted with, where the male flower is separated from the female. These are all strong reasons for believing, that the male plant has been generally mistaken. But there is another source of errour admirably suited to mislead. There is a species of animalcule, which lays its light green eggs very often in some species of mosses; and generally chooses the upper leaves, whence they open to the stalk. These are so like pollen, that it is only keeping them till they hatch, that can prove what they are. I have been twice so deceived. I have added a dissection, at fig. 4, of the stem of the moss, to show the manner in which the spiral wire runs from leaf to leaf at z; and to show the ball, round which it winds at every leaf, thus running up the midrib.

Source of errour in the eggs of animalcules taken for pollen.

II.

Trigonometrical Formulæ for Sines and Cosines. In a Letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

FROM your favourable reception of the Trigonometrical Formulæ, which I had the honour of communicating, and which

which appeared in your Number for February last, I have been encouraged once more to trouble you with a few miscellaneous results, indeed, yet curious.

By the common trigonometrical resolutions of sines and cosines we have: If $\pi = 3{\cdot}1415$ &c.

$$\text{Sin. } A = A\left(1-\frac{A^2}{\pi^2}\right)\left(1-\frac{A^2}{(2\pi)^2}\right)\left(1-\frac{A^2}{(3\pi)^2}\right)$$

$$\left(1-\frac{A^2}{(4\pi)^2}\right)\times \text{\&c. to infinity.}$$

Hence $$A = \frac{\text{Sin. } A}{\left(1-\frac{A^2}{\pi^2}\right)\left(1-\frac{A^2}{2^2\pi^2}\right)\left(1-\frac{A^2}{3^2\pi^2}\right).\ \text{\&c.}}$$

Let now $A = \frac{m}{n}\pi$. And $\therefore\ \pi = \frac{n}{m}.\ \sin.\left(\frac{m}{n}\pi\right)\times$

$$\frac{n^2.\ (2n)^2.\ (3n)^2.\ (4n)^2.\ \text{\&c.}}{(n^2-m^2)\ (\overline{2n}^2-m^2)\ (\overline{3n}^2-m^2)\ (\overline{4n}^2-m^2).\ \text{\&c.}} = \frac{n}{m}.\ \sin.\ \frac{m}{n}\pi.$$

$$\frac{n.\quad n.\quad 2n.\quad 2n.\quad 3n\quad 3n.\quad \text{\&c.}}{(n-m)\ (n+m)\ (2n-m)\ (2n+m)\ (3n-m)\ (3n+m).\ \text{\&c.}}.\ \{1\}$$

Again

$$\text{Cos. } A = \left(1-\frac{A^2}{\left(\frac{\pi}{2}\right)^2}\right)\left(1-\frac{A^2}{3^2\left(\frac{\pi}{2}\right)^2}\right)\left(1-\frac{A^2}{5^2\left(\frac{\pi}{2}\right)^2}\right).\ \text{\&c.}$$

$$= \frac{(n-2m)(n+2m)(3n-2m)(3n+2m)(5n-2m)(5n+2m).\ \text{\&c.}}{n\ .\ n\ .\ 3n\ .\ 3n\ .\ 5n\ .\ 5n.\ \ \text{\&c.}}\ \{2\}$$

In $\{1\}$ let $\frac{m}{n} = \frac{1}{2}$. $\therefore$ sin. $\frac{m}{n}\pi = 1$, and we get $\pi = 2.\ \frac{2\ .\ 2\ .\ 4\ .\ 4\ .\ 6\ .\ 6\ .\ 8\ .\ 8\ .\ \text{\&c.}}{1\ .\ 3\ .\ 3\ .\ 5\ .\ 5\ .\ 7\ .\ 7\ .\ 9\ .\ \text{\&c.}}$. Which is Wallis's expression. This way of deducing it is however far shorter and more direct than the usual way (see Woodhouse's Trigonometry, where, however, he does not seem to have bestowed much attention on this part of his subject).

In $\{1\}$ let $\frac{m}{n} = \frac{1}{4}$ $\therefore$ sin. $\frac{m}{n}\pi = \frac{1}{\sqrt{2}}$, and the form becomes

$$\pi = 2\sqrt{2}\times\frac{4\ .\ 4\ .\ 8\ .\ 8\ .\ 12\ .\ 12\ .\ 16\ .\ 16\ .\ \text{\&c.}}{3\ .\ 5\ .\ 7\ .\ 9\ .\ 11\ .\ 13\ .\ 15\ .\ 17\ .\ \text{\&c.}}.\qquad (a)$$

Let $\frac{m}{n} = \frac{1}{6}$ $\therefore$ sin. $\frac{1}{6}\pi$ being $= \frac{1}{2}$ form $\left\{1\right\}$ becomes

$$\pi = 3 \cdot \frac{6 \cdot 6 \cdot 12 \cdot 12 \cdot 18 \cdot 18 \cdot 24 \cdot 24 \cdot \&c.}{5 \cdot 7 \cdot 11 \cdot 13 \cdot 17 \cdot 19 \cdot 23 \cdot 25 \cdot \&c.} \qquad (b)$$

Let $\frac{m}{n} = \frac{1}{3}$, and form $\left\{1\right\}$ becomes

$$\pi = \frac{3\sqrt{3}}{2} \cdot \frac{3 \cdot 3 \cdot 6 \cdot 6 \cdot 9 \cdot 9 \cdot 12 \cdot 12 \cdot \&c.}{2 \cdot 4 \cdot 5 \cdot 7 \cdot 8 \cdot 10 \cdot 11 \cdot 13 \cdot \&c.} \qquad (c)$$

In the same way by making $\frac{m}{n} = \frac{1}{10}$, or $\frac{1}{20}$, we find

$$\pi = \frac{5(\sqrt{5}-1)}{2} \times \frac{10 \cdot 10 \cdot 20 \cdot 20 \cdot 30 \cdot 30 \cdot 40 \cdot 40 \cdot \&c.}{9 \cdot 11 \cdot 19 \cdot 21 \cdot 29 \cdot 31 \cdot 39 \cdot 41 \cdot \&c.} \qquad (d)$$

And

$$\pi = 5\left\{\frac{\sqrt{5}+1}{\sqrt{2}}\sqrt{5-\sqrt{5}}\right\} \cdot \frac{20 \cdot 20 \cdot 40 \cdot 40 \cdot 60 \cdot 60 \cdot \&c.}{19 \cdot 21 \cdot 39 \cdot 41 \cdot 59 \cdot 61 \cdot \&c.} \qquad (e)$$

and so on, whenever sin. $\frac{m}{n}\pi$ can be found in algebraic terms, as if $\frac{m}{n} = \frac{2}{17}, \frac{1}{17}$, &c.

Let us now take form $\left\{2\right\}$, and for $\frac{m}{n}$ write $\frac{1}{4}$. Now cos. $\frac{1}{4}\pi = \frac{1}{\sqrt{2}}$. $\therefore \sqrt{2} = \frac{1}{\text{cos. } \pi}$, and the form becomes

$$\sqrt{2} = \frac{4 \cdot 4 \cdot 12 \cdot 12 \cdot 20 \cdot 20 \cdot 28 \cdot 28 \cdot \&c.}{2 \cdot 6 \cdot 10 \cdot 14 \cdot 18 \cdot 22 \cdot 26 \cdot 30 \cdot \&c.}$$

$$= \frac{2 \cdot 2 \cdot 6 \cdot 6 \cdot 10 \cdot 10 \cdot 14 \cdot 14 \cdot \&c.}{1 \cdot 3 \cdot 5 \cdot 7 \cdot 9 \cdot 11 \cdot 13 \cdot 15 \cdot \&c.}$$ which is an expression due (if I remember rightly) to Euler.

In form $\left\{2\right\}$ for m write $\frac{1}{2}m$, and it becomes

$$\text{cosin. } \frac{m}{2n}\pi = \frac{(n-m)\ (n+m)\ (3n-m)\ (3n+m)\ \&c.}{n \quad . \quad n \quad . \quad 3n \quad . \quad 3n \quad . \ \&c.}$$

Let $m = 1, n = 3$. $\therefore$ cos. $\frac{1}{6}\pi$ being $= \frac{\sqrt{3}}{2}$ we get

$$\sqrt{3} = 2 \cdot \frac{2 \cdot 4 \cdot 8 \cdot 10 \cdot 14 \cdot 16 \cdot 20 \cdot 22 \cdot \&c.}{3 \cdot 3 \cdot 9 \cdot 9 \cdot 15 \cdot 15 \cdot 21 \cdot 21 \cdot \&c.} \qquad (f)$$

Let $m = 4$, $n = 5$. $\therefore \cos. \frac{4}{10} \pi = \cos. \frac{2}{5} \pi = \frac{\sqrt{5} - 1}{4}$ &c. :

$$\sqrt{5} = 1 + 4 . \frac{1 . 9 . 11 . 19 . 21 . 29 . 31 . 39 . 41 . 49 . \&c.}{5 . 5 . 15 . 15 . 25 . 25 . 35 . 35 . 45 . 45 . \&c.} \quad (g)$$

In the same way as Euler's theorem, $\frac{A}{2} = \sin. A - \frac{1}{2} \sin. 2 A + \frac{1}{3} \sin. 3 A -$ &c. is deduced, we may obtain the following

$$\left.\begin{array}{l} \cos. A + \cos. 3 A + \cos. 5 A + \&c. \text{ to infinity, always} = 0 \\ \cos. 2 A + \cos. 4 A + \cos. 6 A + \&c. \text{ always} = -\frac{1}{2} \end{array}\right\} \quad (h)$$

and $\therefore \cos. A - \cos. 2 A + \cos. 3 A -$ &c. $= \frac{1}{2}$ as may also be had by differencing Euler's series.

Again, if $e = 2{\cdot}7182818$ &c. we find

$$\left.\begin{array}{l} 2 . \cos. \frac{A}{2} = e^{\cos. A - \frac{1}{2} \cos. 2 A + \frac{1}{3} \cos. 3 A - \frac{1}{4} \cos. 4 A + \&c.} \\ \text{and } 2 . \sin. \frac{A}{2} = e^{-(\cos. A + \frac{1}{2} \cos. 2 A + \frac{1}{3} \cos. 3 A + \frac{1}{4} \cos. 4 A + \&c.)} \end{array}\right\} (i)$$

Again $\frac{\pi - A}{2} = \sin. A + \frac{1}{2} \sin. 2 A + \frac{1}{3} \sin. 3 A +$ &c. (k)

And $\frac{\pi^2}{6} - \frac{\pi A}{2} + \frac{A^2}{4} = \cos. A + \frac{1}{2^2} . \cos. 2 A + \frac{1}{3^2} .$
$\cos. 3 A + \frac{1}{4^2} . \cos. 4 A +$ &c. (l)

And again $\frac{A}{2} (\pi - A) = \frac{(\sin. A)^2}{1^2} + \frac{(\sin. 2 A)^2}{2^2} + \frac{(\sin. 3 A)^2}{3^2}$
$+ \frac{(\sin. 4 A)^2}{4^2} +$ &c. (m)

These last theorems are so easy of deduction, that I have omitted their demonstrations for the sake of keeping within the compass of a letter.

I am, Sir,

Your most obedient humble servant,

March the 23d, 1819 ANALYTICUS.

III.

Inquiry concerning the Means of studying the Modern Analysis. In a letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

AS the object of your excellent Journal is the diffusion of scientific knowledge among all classes, I am sure you will not deem the present queries out of place; and if you will have the goodness to reply to them either by private communication, or through the medium of your publication, you will confer an obligation not merely on the individual who addresses you, but upon many others in the same circumstances as myself.

What books are necessary to the study of the modern analysis?

The object of my inquiry is this:—What elementary works should be perused by a person who wishes to become acquainted with what is usually termed "*the modern analysis*"? That one who resides in a Mathematical University should put this question may appear strange; but it is well known by many, who, like myself, have devoted a considerable portion of time to the study of mathematics according to the system adopted in this university,—that so little attention is paid to the *modern language* of science, that the most admired works of the foreign Mathematicians are a dead letter even to many of those, who are sufficiently familiar with the works of Newton and the ablest English philosophers.—Suppose then that a person is tolerably acquainted with pure *Geometry*, and with the *fluxional* calculus, what course of reading should he pursue, in order to qualify himself for the perusal of *La Place's Mécanique Céleste*?

As these observations are addressed to you by one who is an enthusiast in mathematical studies, but who knows of no other means of getting satisfactory information upon the subject of his inquiry, than that which he has adopted, an early reply would be extremely acceptable.

A. H. Z.

Answer.

Works recommended for the study of the modern analysis.

With respect to the books my correspondent inquires after, I would recommend, as the first and principal, the *Traité du Calcul Différentiel et Intégral* of Lacroix; which, with the qualifications he mentions himself as possessing, will be sufficient to give him a very complete notion of most of the branches of the *modern analysis*. He should, however, read with great attention, before he begins to look into the *Mécanique Céleste* of La Place, the *Traité de Mécanique Elémentaire* of Francœur, which is an excellent introduction to that work, and the *Mécanique Analytique* of La Grange, which is a work of the first rank in this department of science. If to these he joins the *Théorie des Fonctions Analytiques*, and *Leçons sur le Calcul des Fonctions*, by La Grange, he will be able to proceed, with great ease, in any undertaking of this kind, that he may wish to engage in; these being, as I conceive, all the most necessary and useful performances, that have hitherto appeared on the subject of what is more peculiarly called the *modern analysis*.

IV.

Experiment to prove, whether Water be produced in the Combination of Muriatic Acid Gas and Ammoniacal Gas. By JOHN BOSTOCK, *M. D., Vice Pres. of the Lit. and Phil. Soc. of Liverpool, and* THOMAS STEWART TRAILL, *M. D., Secretary to the Society. Read before the Literary and Philosophical Society of Liverpool, and communicated by Dr.* BOSTOCK.

To Mr. NICHOLSON.

SIR,

Mr. Murray's attempt to prove the existence of water in muriatic gas.

IN your Journal for February, Mr. Murray has related an experiment, which he performed on the mixture of muriatic and ammoniacal gasses, the object of which was to ascertain, whether, when the gasses were added together in the

the state of perfect dryness, the muriate of ammonia, formed by their mixture, contained water. A very obvious quantity of water was expelled from the salt, and it was argued, that this water must have formed a constituent part of the muriatic gas, for it is now agreed, that pure ammonia consists entirely of hidrogen and azote; and from the terms of the experiment it is supposed, that all moisture was removed from both the gasses, and excluded from every part of the apparatus. In your Journal for March, a correspondent, who signs himself A. B. C., undertakes to set aside the inference from the above experiment. This he does, not by showing that either of the gasses, or any part of the apparatus, contained water, nor by denying the existence of water in the result of the process as conducted by Mr. Murray, (for these points appear to be admitted) but by attempting to prove, that the muriate of ammonia had attracted moisture from the atmosphere, while it was transferred from the vessel in which it was originally formed, into the one to which the heat was applied; and to prove this he relates an experiment, in which newly formed muriate of ammonia attracted water, simply by being "removed through the atmosphere "into a dry tube."

The moisture said by a correspondent to have been attracted from the atmosphere.

The experiments are in themselves curious, and are at this time particularly interesting, as forming a part of the controversy respecting the constitution of muriatic acid. From these considerations Dr. Traill made a proposal, to which I very willingly assented, that we should in conjunction repeat the experiments of Mr. Murray and the correspondent; that we should especially attend to every circumstance, by which moisture might be excluded; that the muriate of ammonia formed should be heated, without being at all exposed to the air; and that the quantity of moisture, which it acquired from exposure to the atmosphere, should be accurately ascertained. Before we entered upon the process we resolved, that, provided no circumstance occurred to interrupt or defeat the experiments, the results, whatever they were, should be communicated to your Journal.

The experiments carefully repeated.

Every circumstance as to the cleaning and drying the different parts of the apparatus, and the providing of the necessary substances, being attended to, we commenced our

Preparation of the muriatic gas.

 operations

operations by the preparation of the muriatic gas. Two ounces of muriate of ammonia, in coarse powder, and which had been kept heated for two days, were mixed with 9 drachms by measure of sulphuric acid, of the specific gravity of 1·85, in a tubulated retort. The gas soon began to form, without the assistance of heat; and, after a considerable quantity had escaped, we received a portion of it over mercury. The gas was perfectly transparent and colourless, no moisture was perceived within the jar, and none was visible in any part of the retort; it was indeed observed, that some particles of the muriate of ammonia, which had lodged on the lower part of the neck of the vessel, remained perfectly dry at the end of the process. A quantity of muriate of lime, perfectly dry and pulverulent, was introduced through the mercury into the muriatic gas, and in this state it remained for 48 hours.

Preparation of the ammoniacal gas.

The ammoniacal gas was prepared by introducing into a retort equal weights of newly burned quick lime and muriate of ammonia, in the same state with that used above. By means of a lamp gas was expelled, and after a sufficient quantity had escaped, a portion was received over mercury. When the jar was become cold, a little dew was perceived on the upper part, which was very carefully removed by bibulous paper, introduced on the end of a wire. A considerable lump of dry quick lime was then placed in the gas, and was suffered to remain for 48 hours.

Mixture of the gasses.

At the end of this time we resumed our operations. Upon the closest inspection we could not perceive the least moisture in either of the gasses, or appearance of it in the jars; the lime and the muriate of lime were withdrawn, and it was observed, that the latter was to all appearance as dry as when it was first introduced. A flask, furnished with a ground stopper and bent tube, had 13 cubic inches of ammoniacal gas introduced into it over mercury, and to this was added 6 cubic inches of muriatic gas in successive portions. The flask was then entirely filled with ammoniacal gas, and the apparatus was left at rest for about an hour; it was coated, more especially at its lower part, with a fine frost work of muriate of ammonia. The stopper and tube were then introduced, and the flask was turned over, but so that

Muriate of ammonia formed, and,

that the end of the tube was kept below the surface of the mercury, so as to exclude all communication with the atmosphere. The flask was then embedded in a charcoal furnace, and gradually heated, until it was softened. This process continued about an hour, when the muriate of ammonia was all sublimed into the neck of the flask, or into the commencement of the tube. When the salt was about half sublimed, a dew was observed to form at the upper part of the curvature of the tube, about an inch from the stopper. This dew increased, so that at one period it occupied a zone all round the tube of about an inch in width, and some globules of water were formed of about the size of a small pin's head. Towards the end of the experiment, as the heat increased, the dew was diminished; but when the tube was removed from the mercury a similar deposition of moisture was observed at the end, where it had been immersed in the metal. Before it was taken from the mercurial bath the tube had its aperture luted with wax, in order to exclude all communication with the atmosphere, which was farther ensured by a globule of mercury being lodged in the curvature of the tube; and, as soon as it was become cool, the flask was opened, a part of the salt scraped from the neck, and weighed as quickly as possibly. The quantity collected was 2·7 grs, and not more than a minute could have elapsed between its being removed from the vessel, and its weight being ascertained. It remained in the scale for 15 minutes; but although we thought that the index of the balance rather inclined to that side, no increase of weight could be positively asserted to have taken place. In order that a judgment may be formed of the delicacy of the instrument, we found it to turn with $\frac{1}{10}$ of a grain, when each side was loaded with 500 grains.

without having been at all in contact with the atmosphere, sublimed in the same vessel.

A dew formed,

and moisture deposited at the end.

Part of the salt taken out and weighed quickly,

and it gained no perceptible addition of weight by exposure to the air 15 minutes.

I am, sir,

Your obedient servant,

J. BOSTOCK.

Knot's-hole Bank, near Liverpool,
March the 26th, 1812.

V.

V.

Questions respecting a Passage in Mrs. Ibbetson's *Account of the Water Lily. In a Letter from a Correspondent.*

To W. NICHOLSON, Esq.

SIR,

Ambiguity in Mrs. Ibbetson's account of the water lily.

IN the last number of your very excellent publication, there is a paper of Mrs. Ibbetson's, in continuation of her valuable discoveries in the minute anatomy of plants, in which I would, though with the greatest deference, point out an ambiguity, in my opinion of considerable importance. The passage I allude to is in the description of the structure of the water lily, page 243, where, after referring to "*a. a.* of fig. 1, Pl. VII," for a view of the *air vessels*, she says, that, lest the pith "should not be sufficient to prevent insects from entering into it, and choking up the air vessel, as soon as the plant sinks in the water, a quantity of hairs, which are placed in circles in the interior, rise, and, meeting in the centre, not only aid to keep out the water, but run through every insect, that ventures to approach." Mrs. Ibbetson then goes on to add, "I have often caught insects threaded on the hairs, but they are soon washed off.

Questions respecting it.

Now a question or two naturally arise on reading this observation.—1st, how do insects get *into* or even *near* the air vessels? or, 2ndly, how can the water come at them, to wash them off, when these vessels are so entirely internal?

I doubt not that these questions can be most satisfactorily answered; but, certainly, Mrs. Ibbetson did not show her usual perspicuity in this passage.

Your insertion of this, or an answer to it, if possible in your next, will very much oblige me.—I conclude by sincerely thanking the lady, who is the occasion of this letter, for the high gratification, which her frequent communications have afforded—and with hopes, that she will persevere in her truly wonderful and interesting discoveries,

I am, sir,

Your much obliged, and very humble servant,

Poole, April the 4th, 1812. T. B.

VI.

VI.

The Stater of Philip, the Father of Alexander; or Remarks on the Purity or Standard of Gold: By Mr. J. FABBRONI, *of Florence, Corresponding Member of the French Institute**.

NATURALISTS, perhaps on the authority of Pliny (1)†, are almost unanimous in the assertion, that native gold is never found perfectly pure, or free from all alloy, particularly of silver; and that the finest is scarcely from 0·875 to 0·917, that is from 21 to 22 carats. The gold dust brought from Africa is commonly within these limits. I have seen some at 0·927, or 22 carats and a quarter‡; and lately there has been some at 0·958, or 23 carats, brought from Morocco to the mint at Florence. (In Tuscany the carat is divided into eighths.)

Native gold said never to be pure.

Gold dust from Africa.

It is probable, that in the early ages money was coined of native gold, in the state in which it was found; for there could be no inducement to incur the trouble and expense of refining it.

Ancient coins from native gold.

The most ancient gold coin known is supposed to be that of Battus IV, cast or struck at Cyrene, in Africa, in the time of Pisistratus. Its fineness does not appear to be known. Of all the Grecian coins found in our cabinets of medals the most ancient are the beautiful pieces of Philip, father of Alexander. This enterprising man, who from his infancy looked forward to ascend the throne of Macedon and become master of Greece, had the good fortune to find some rich gold mines, which he knew how to work to great advantage. Mount Pangæus furnished him annually to the amount of 6300000 Florence liri [£218750]. Hence he derived the most powerful instrument of the success of his political designs and military talents. Whether the gold of Philip underwent any particular operations, before it was

Most ancient known.

Oldest Greek, those of Philip.

His mines.

* Ann. de Chim., vol. LXXII, p. 25.

† The figures refer to notes by Mr. d'Arcet at the end of the paper.

‡ This gold is found chiefly in the country of Bambouck.

sent

sent to the mint, is not known; but there is reason to believe, that it was employed in the state in which it was found*.

Assay of his stater.

Patin assayed a gold stater of this king, and found it 23 carats and a half fine, or 0·979: and, as it cannot be supposed, that his mintmen would have thought of purifying gold, to add afterward no more than a forty-eighth of alloy, we may presume, that the gold was found native of this fineness.

Addition of alloy to gold.

If alloy have been added to gold with a bad design, or with the erroneous idea of defraying the expense of coinage; it is a remedy that has degenerated into fraud, and has no limits. If alloy have been added with the design of rendering the coin harder, it is a useless idea. Neither of these motives could have induced Philip to adopt the practice, because the source of his gold was abundant, and he was desirous of appearing generous; so that he would have coined his money of pure gold, if he had thought it necessary to refine it; or he would have added more alloy, if policy had suggested to him, not to employ it in the virgin state, as it came from the mine (2). It would appear therefore, that nature furnished him with gold at 23 carats and half, or 0·979, as it is in his coin; unless there were an errour in the assay of Patin, which deserves therefore to be verified.

Philip used his gold native.

A stater lately found.

The chevalier Fossombroni, a very celebrated mathematician, digging the foundations of a house near Arrezzo, found a stater of Philip in very good preservation. No sooner was he informed of the wish to examine the weight and chemical composition of his antique, than he readily sacrificed it to the gratification of this curiosity.

Described.

The obverse of this piece, like that of most of Philip's coins, bears the head of Apollo; and the reverse, a chariot with two horses walking. The name is in the exergue. On similar staters under the legs of the horses appears a monogram, or some type, to denote the mint where the piece was struck. On this stater it is a trident, the symbol of Trœzene.

* Pliny hints, that gold was found in the bowels of the earth sufficiently pure, to be melted without any preparation.

Fourteen

Fourteen staters of Philip are preserved in the rich cabinet of the Florence gallery. Eleven resemble that of Arrezzo on both sides, but they have different mint-marks; one only having the same as that found near Arrezzo. The weight of two of these staters, perfectly resembling each other in external appearance, is precisely 176 Florence grains [133·6 grs Eng.] This is precisely the weight of another stater, the mint-mark of which is formed by a large K, and a small o; of one that has a thunderbolt; one with a vase; and one with an ear of corn, the mark of the Leontini. This being the weight of the six largest staters that have come down to us, there is reason to presume, that it was the weight prescribed for this Greek coin*. Hence it may be inferred, that the drachma was equivalent to 88 Flor. grs. [66·8 grs E.]. (De Romé-de-Lisle gives 4·461 gr. [68·9 grs] for the great attic drachm, that is to say, about 2 grs more.) A proof of the justness of this weight is the attic hemidrachma, or Asiatic drachma, or fourth part of the stater of Philip, which is also preserved in the same gallery, and weighs precisely 44 grs. [33·4 grs E.]. The obverse of this small piece of gold bears the head of Hercules covered with the lion's skin. On the reverse are the bow, vase, and club. The learned and illustrious professor A. L. Millin has sent me the weights of five Philippi in the imperial library; which are as follows. No. 1, 160·5 grs: 2, 161 grs very exactly: 3, 161 grs: 4, 162 grs very exactly: 5, 162 grs. The two heaviest, which differ by an unassignable fraction, are so because they are least worn. The heaviest answers to 175·16 Flor. grs., and is therefore 0·84 of a grain lighter than ours; which therefore may be considered as less worn, and more accurate.

Fourteen in the Florence gallery.

Their weight.

Weight of the drachma.

Attic hemidrachma.

Weights of 5 staters in the imperial library.

Greaves weighed two staters of Alexander, one of which was 133 grs English, the other 133·5. He supposed, that the half grain had been lost by wear; and he concluded, that the drachma should be estimated at 67 grs precisely. The second weight given by Greaves is equivalent to 87·6 Flor. grs. Snellius found the stater of Philip, and of Alexander, to weigh 179 Dutch grs, equivalent to 124·5

Staters weighed by Greaves,

and by Snellius.

* No heavier stater is known to exist.

Eng.;

Eng.*; which, from a comparison with the preceding, would give for the drachma 87·9 Flor. grs; still a little lighter, but very near what we have assigned, or 88 grs.; without its being necessary to estimate the wear, in support of six similar weights in an equal number of gold staters, and with the proof of the fraction mentioned. The celebrated Barthelemi found, from various weighings, that the drachma was precisely 81¼ French grs [66·55 grs E.], which would give about 87·75 Flor. grs. But he would presume a loss of seven eighths of a grain for the wear of 2200 years, and thus gratuitously make the drachma equal to 82 Fr. grs, or 88·5 of ours. It is probable however, that he carries his estimate too high. We should altogether reject from our calculations all allowance for wear; because, by admitting this, we may draw any vague conclusions we please. The weight of 88 grs [66·8 grs E.] is confirmed by a silver drachma of the same Philip, likewise preserved in the Florence cabinet. On the obverse is the head of Hercules, without a beard, and covered with the lion's skin; and on the reverse Jupiter seated, with the eagle on his right hand, and a spear in his left. It is distinguished from others by a lyre and the letter A beneath the seat. The accuracy of the weight of this drachma is confirmed by its half, also in silver, of the same king, which weighs exactly 44 grs. This has the head of Jupiter, ornamented with the diadem; and on the reverse is a figure on horseback, with the name in the exergue, and a mark that cannot be made out. Besides, there are four tetradrachmas of Alexander, of the same metal, the faces and reverses of which are similar; which, weighing all alike 14 den. 16 grs, farther prove the weight of the drachma to be 88 grs. These tetradrachmas are distinguished by various marks, as was said of the staters. One has in the fore part a lamp, and under the seat a moon and a star: another has in the same place the initial T with a circumflex over it, and under the seat the letter E; a third has a buckler, and under the seat a serpent: the fourth has a crown, and under the seat a monogram, composed of an

Weight according to Barthelemi.

Silver drachma of Philip.

Silver hemidrachma.

Tetradrachmas of Alexander.

Mint marks.

* There is evidently some mistake here; but, as I do not know the precise weight of the Dutch grain, I shall leave it as in the original. C.

M barred between the two inner strokes. Lastly we have also a real drachma of this king, of the precise weight of 88 grs, which is distinguished by a monogram, consisting of an H, with a kind of circumflex over the cross stroke. Drachma of Alexander.

Among the tetradrachmas of Thrace in the same cabinet there is one, the twelfth in order, heavier than the rest; and weighing precisely 14 den. 16 grs. This is a proof of the identity of the weight of the Thracians and Macedonians, which had already been conjectured by others*. Thracian tetradrachmas.

After having ascertained the weight of the Philippus found at Arrezzo, it was subjected to cupellation, and the process of parting. Its fineness appeared to be the same as found by Patin; that is 0·979, or 23 carats and a half; containing but half a carat, or 0·021 of silver. The stater assayed.

The art of assaying was known in the remotest times, as the Scriptures attest. In the time of Pliny it had reached such perfection (3), that the fineness of gold was ascertained from 21 carats, or 0·875, to 21 carats and 7 twenty-fourths, 0·888, and even to 23 carats and 11 thirty-seconds, 0·973. In those days the assay must have been made in the dry way; first by separating the base metals from the gold by means of lead, and afterwards the silver by means of sulphur†, or a sulphuret (4). Art of assaying ancient.

The method of refining gold in large quantities was also known, as Strabo says, by cementing or burning it with an argillaceous earth, which, *destroying* the silver, left the gold in a state of purity. Pliny says, that for this purpose the Ancient art of refining. Strabo. Pliny.

* The scholiast on Nicander says, that the didrachma is the fourth part of the Attic ounce: this ounce then must be 704 Flor. grs. [534·4 grs. Eng. Here, as in the other parts of this paper, I have reduced the Flor. grs directly into Eng., agreeably to the values assigned them by Tillet, in the Mem. of the French Academy of Sciences for 1767; paying no regard to the reduction into grammes, made I presume by the French translator, and added in the Ann. de Chim. He gives here 34·496 grammes as equivalent to 704 Fl. grs, which would then be only 532·8 Eng. C.]

† A manuscript written by one Biffoli, who lived in 1460, which is in the Strozzian library, and of which there are several other copies, says: "Parting with aqua fortis was invented about fifty years ago."

gold

gold was placed on the fire in an earthen vessel with three times its weight of salt; and that it was afterward exposed anew to the fire with two parts of salt, and one of *schist*, certainly argillaceous. This would certainly effect the decomposition of the salt, and the volatilization of the muriatic acid in a state of ignition and dry, which would penetrate the substance of the gold, and separate the silver in the form of a volatile muriate; the object (5) and effect of the cementation of the moderns. But Agatharchides has transmitted to us an account of a peculiar method practised in the mines situate between the Nile and the borders of the Red Sea*, in which we perceive the well known property of the muriatic acid in separating silver.

Agatharchides.

His description.

This author says, if he express himself accurately, and there be no corruption of the text, that the gold there is enclosed in marble: that the miners burn or calcine this ore: that they break it with hammers, pound, grind, and wash it: and that lastly the gold, placed in a covered crucible with a little lead, some salt, a little tin, and some barley-meal, was exposed to the fire five days.

Pure gold coins of Darius.

The mintmen of Darius certainly employed this or a similar method, when this enlightened king† was desirous of giving his subjects the noble and useful example of money made with the purest gold, similar to that of fine silver made afterward by his satrap Ariander.

Process of Agatharchides difficult to explain.

It is not easy, however, to give a plausible explanation of the rationale of the docimastic method transmitted to us by Agatharchides. But if the operation he describes were intended not as a *cementation*, but a real and prolonged fusion, it remains to be explained, how the employment of a closed crucible, kept on the fire as he directs, is to be reconciled with the object proposed: nor is it easy to comprehend the use of barley-meal.

A similar method apparently practised at Lyons.

But on reflecting on the ingenious method, which Hellot found practised at Lyons, for refining, purifying, and separating cupelled silver from the little lead that remains with

* Gold was extracted from these mines even previous to the discovery of iron.

† The scholiast on Aristophanes ascribes this to another prince of the same name, but more ancient.

with it after the first refining, we may form some notion of it (*).

The practice in that city was to take a crucible thirteen inches high, and five inches wide at the mouth: to put a layer of small charcoal three inches deep at the bottom, and cover it with a triangular piece of a crucible, fastened by a little lute at each corner, its sides answering to the corners of the crucible: and on this false bottom to place sixty or sixty-five pounds of silver in long slender ingots, to be melted and purified. The wind-furnace used for this purpose was fourteen inches high, seven in diameter at the grate, and nine at the top. The metal, as it melted, was observed to sink to three inches below the edges of the crucible; and then, when it had acquired a sufficient degree of heat, it was seen to boil like water exposed to a strong fire. In this state it was kept seven or eight hours.

Described by Hellot.

The elastic fluid, which in this case was evolved from the charcoal beneath, caused the agitation here mentioned; the charcoal constituting, as we may say, a kind of bellows ingeniously placed at the bottom of the crucible.

Artificial bellows.

Charcoal, placed in close vessels of glass or metal, we know is not altered, though heated redhot. This we are taught by theory, and the truth is confirmed by many experiments. But the observation reported by the judicious Hellot equally attests, that in this case the charcoal beneath the melted silver is decomposed, and continues to furnish elastic fluid; since this learned chemist found, that silver kept in the same degree of heat, without any charcoal beneath, has a tremulous motion at its surface, and proceeds from the centre to the sides and back again, but does not boil with such noise*: whence then comes the elastic fluid?

Charcoal not decomposed in close vessels of glass or metal: but it is in earthen ones.

Priestley, the founder of modern pneumatic chemistry by an immense number of facts, demonstrates in the most evident manner, what has since been confirmed by many other experiments, that earthen vessels, heated to such a degree as to give a passage to light, are filters, or rather sieves,

This found by Priestley.

* The silver has merely an undulating and circulatory motion.

giving

The process explained on this principle.

giving admission even to the external air*. Thus caloric and light penetrating the bottom of the crucible, and with them the air, attracted chemically by the charcoal within, its oxigen, coming into contact with the incandescent charcoal, inflames a portion of it, combines with it and caloric, and forms carbonic acid. This elastic fluid, through the uninterrupted action of the fire, acquires sufficient force, to overcome the pressure of a column of seven inches of liquid silver above it, and passes through it, agitating it violently. The small residue of lead, which was combined and diffused throughout the mass, being brought by the continual agitation into contact with the carbonic acid gas and the atmosphere (the latter, and perhaps the former, being decomposed by a superior affinity from the concurrence of circumstances), is oxided, and, from the diminution of its specific gravity, is compelled to occupy the upper surface.

The fused oxide of lead rose like an oil.

In fact, Hellot observed a kind of yellowish oil rise from the interior of the melted silver, and float on it. This oil was a pure oxide of lead in fusion; formed by the contact of the continually renewed atmospheric air. The refiners collect this melted oxide, by enveloping and absorbing it with glass or a meagre earth; this earth being removed more readily from the silver it covers, and then the metal remains pure and limpid.

The process of Agatharchides similar.

If we refer to this method the process of Agatharchides, reported above, though very imperfectly, we may suppose, that the barley, or its meal, was employed instead of charcoal, to form what the Lyonese call *the soul of the crucible*; that it was placed at the bottom of the crucible, and retained there by a cover (whence probably the expression of a *closed crucible*); and that on this was poured the gold fused with a little lead, to vitrify the base metals it might contain, and common salt, and sulphuret of antimony or of lead, to seize

* This is denied by many able chemists, who assert, that Priestley was mistaken in his idea; and that the air, in his experiments, was admitted through minute cracks in his vessels, imperceptible to the naked eye. Still this does not invalidate the reasoning of Mr. Fabbroni; for, if this be the true state of the case, air might be admitted to the charcoal in this process through similar cracks in the bottom of the crucible. C.

the

the fine silver, and volatilize it with the lead, or reduce it to scoriæ. The elastic fluids evolved from the vegetable matter by the action of the fire would perform the office of bellows, to agitate the metal violently and incessantly for several days, which would occasion all the impurities to float on the surface, where they would be scummed off as is done by the Lyonese.

Objection. But, to say the truth, a fire continued for five days gives rather an idea of the cementation of the moderns, analogous to that transmitted to us by Pliny, than of a real fusion in closed crucibles; a circumstance directly opposite to the purpose intended. Hungarian process. Thus in Hungary, the better to open all the interior parts of the gold to the muriatic acid reduced to vapour in the process of cementation, it is customary to add lead to the mass, which is afterward reduced into small hollow drops, or grains as they are called. The process of Agatharchides explained by this. It is possible, that the lead mentioned by Agatharchides was intended for the same purpose; that tin is a mistaken expression for crude antimony, or native sulphuret of lead; and that the barley meal was intended merely to promote the uniform distribution of the little salt, a stratum of which was to be placed on the gold, and assisted perhaps in decomposing it, as clay or sulphate of iron does now.

Experiment to prove its effects. To obtain some light on this curious subject, into a crucible, covered by another inverted over it, were put 720 grs of barley meal, and 576 grs of common salt. This mixture was heated till it acquired the colour of a redhot coal, and in this state it was kept for six and thirty hours. More from curiosity, than to derive any important conclusion from it, into it had been put a small slip of gold, at 21 carats 3 eighths, or 0·891, a third of a millimeter [about 0·13 of a line Eng.] thick, and weighing 24 grs; and a slip of silver, at 11 dwts and half, or 0·958, half a millimeter [near 0·2 of a line] thick, and weighing 40 grs. The lower crucible, in which these were placed, was half full; and in the luting of that above was left an opening of 5 mil. [near 2 lines] for the issue of the elastic vapour.

Results. At the expiration of this time the apparatus, after being cooled, was opened. In it was found a very little earthy residuum, slighly saline, whitish, weighing scarcely 11·5 grs.

grs. The gold was above it, and increased in weight an eighth of a grain, being perceptibly whitened by the fusion of some very small particles of silver, separated from the remains of the little slip of that metal, which was found sticking upon the gold in the form of an agglutinated dust possessing very little adhesion. These remains were pure silver, and weighed 6 grs and an eighth. The gold, which was silvered only on its surface, was boiled some time in pure nitric acid; when it lost entirely its silvery hue, and was found, on assaying it, to be of 24 carats.

Earthy residuum.

The little earthy residuum was then examined. In it were found no saline particles but a few atoms of muriate of soda, and barely a trace of muriate of copper. The muriate of silver, which from the loss of the metal must have weighed 45·5 grs, had certainly evaporated with the other elastic vapours. In the formation of this muriate only 11·5 grs of muriatic acid had been employed. The 324 grs of acid beside, contained in the salt employed, were dissipated (leaving the small portion of copper out of the question) by a decomposition effected through the means of the vegetable matter mixed with it. But what is difficult to account for, and is foreign to our purpose, is the entire evaporation of 240 grs of soda, which the common salt contained, and which should have remained fixed at the bottom of the crucible. This must have been rendered volatile either by decomposition, or by forming a new compound, and escaped through the opening in the apparatus.

Evaporation of the soda.

Philip used native gold.

It is not probable therefore, that Philip employed similar methods of refining, either by fusion or by cementation, because, I must repeat, he would have reduced the gold to a state of perfect purity, as Darius thought proper to do subsequently; or he would not have confined himself to so small a portion of alloy, or perhaps that alloy would not have been silver. And if he employed the gold as he found it, we must necessarily infer, that nature yields gold at 23 carats and half, or 0·979 (7).

Doubts respecting the fineness of native gold:

Many perhaps will doubt, whether gold be found in nature so near to perfect purity; though Strabo says, that gold was found pure in the Noric Alps; while Pliny is quoted for the assertion, that none is found free from silver. But,

But, without being left in suspense by the assertions and opinions of others, I have the means of removing all doubt on the question; having had an opportunity of ascertaining by my own examination, that gold is actually found native at 24 carats.

but it is sometimes absolutely pure.

I had for some time the keeping of the rich collect ion of natural history belonging to our first king, who was very fond of these things, and eminently versed in natural philosophy.

Collection at Florence.

In it were many specimens of mineralized gold and native gold, among which I observed two well formed crystals of gold; one cubical, the other a tetraedral prism surmounted by a four-sided pyramid. It would be gratifying to know what substances united to the gold determined these different figures, naturally formed in the bowels of the Earth, and altogether different from those produced in our laboratories by cooling after fusion. The cube is very pale; the prism is of a deeper colour: but these two crystals, which I found by chance in selecting a great many native grains, are unique in the collection, so that it is impossible to think of subjecting them to an examination, that would spoil their figure.

Specimens of native gold in it.

Two singular crystals.

An amorphous but remarkable specimen from Brazil enriched the same collection. It was given by the Prince of Brazil, at Badajoz, to the late king of Etruria, then infant of Spain and hereditary prince of Parma. The weight of this piece is about 14 lbs. [12 lbs, 9 oz. troy]*, beside a small fragment of the same, the nature of which, through the kindness of the king's apothecary, John Ulrici, I was enabled to examine by cupellation and parting; without neglecting to test its solution in nitromuriatic acid by sulphate of iron, and neutral salts with base of potash. By all these trials I was convinced, that it is very pure gold of 24 carats, if the whole mass be homogeneous, without any portion of inferior metal.

Specimen from Brazil, weighing 12 lbs, 9 oz.

Part of it examined was perfectly pure.

As no person has ever doubted, that very coarse gold is

* Pliny informs us, that pieces above ten pounds weight were called by the Spaniards in his time *palacras* and *palacranas*; others say, that small pieces were termed *palas*, whence perhaps our *pagliette*, and the French *paillettes*.

 found

found in minerals containing it, I am now certain, that nature likewise presents us with it of the greatest fineness, and even perfectly pure. This is what I purposed to show by this new fact, in writing this little essay, as a present to the lovers of mineralogy and antiquities.

Notes on the preceding Paper by Mr. D'ARCET, *Verifier of Assays at the Mint of France.*

Pliny's testimony.

(1) Pliny says, book 33, that there is no gold more pure than that obtained from the sands of rivers; and that all gold obtained by arrugiæ has no occasion to be melted, being pure native gold. But Pliny says in the same book, that lead is more malleable and heavier than gold, which is a mistake, and proves, that the gold considered by Pliny as pure was an alloy. He says also farther on, that all gold is mixed with silver; and that the freest from silver known is the gold of Albicrare in Gaul, which contained but a thirty-sixth*: whence it follows, that the testimony of Pliny to this point is of no weight, and that we must appeal to experiment.

Analysis of an ancient coin of Philip.

(2) I delivered to Mr. Mongez the analysis of an ancient coin with the effigies of Philip, which proves, that in his reign coins were made of alloys, the composition of which was native, or at least unknown; for this piece contained

Silver	368
Gold	184
Copper	448
	1000.

It is not probable, that the regulations of the mint required such or so complex an alloy, at a time when the methods of analysis or assay were but approximations; and when they were unquestionably far from the accuracy, that may be obtained even by employing only the touchstone, touchneedles, and prepared acid, used at present.

The art of assaying among the ancients

(3) The art of assaying was as far as possible from perfect in those remote periods. Under the emperors even the fine-

* He speaks of other gold containing a tenth, a ninth, and even an eighth part. C.

ness

ness of gold and of silver was judged by the colour it took in the fire, and that of its streak on the touchstone. very imperfect.

These methods, though practised by experienced men, can give only very inaccurate results, and which may be varied by a number of circumstances; as strong cleaning by aqua fortis, a complication in the alloy, a difference in the alloy, &c.

Archimedes would not have applied the laws of specific gravity to ascertain the falsification of the crown of Hiero, if he could have done it by a better method, and particularly by a method known and commonly practised.

It is well known too, that, under the triumvirate of Mark Antony, every street in Rome erected a statue to Marius Gratianus, who had invented and introduced one of these approximative methods, that have been mentioned; and this denotes the infancy of a useful art, the first steps of which are highly encouraged, because they are considered as conducive to the public welfare.

(4) By employing alkaline sulphurets the solution of gold may be effected: metallic sulphurets only must be understood here. Sulphurets.

(5) Mr. de Robilant, in his account of the processes employed in the mint of Turin, says, that cementation is the process of refining commonly employed at Venice, Genoa, and Florence, where zechins are coined of nearly pure gold. Italian mints.

(6) As Mr. Fabroni says, it is not easy to explain the grounds of the process described by Agatharchides, or of that which appears to be still practised at Lyons. These processes should be repeated, attending to their progress with care, and applying to them the means of modern chemical analysis, particularly the pneumatochemical apparatus. The nature of the gas that traverses the fluid silver should be ascertained, why it forms under such a pressure, why it does not flow back through the pores of the crucible, &c. Process of Agatharchides.

The experiment related by Mr. Fabroni does not appear to me sufficiently conclusive, to decide the question.

(7) Reaumur says, Mémoires de l'Ac. des Sciences, An. 1718, p. 87, that Fineness of gold dust in Europe.

The gold of the river Cèze is at 18 car. 8 grs
Rhone.... 20
Rhine .... 21·25
Arriège.... 22·25.

Lumps of native gold not homogeneous.

He farther observes, that the fineness varies in the same piece of native gold. He says, that the piece of 56 marks, which was seen at the Academy, was in one place 23 carats and half, in another 23 carats, and in another 22. The piece of 63 marks belonging to father Feuillée was at its upper part 22 carats 2 grs; a little lower, 21 carats, ½ gr.; and at two inches from the bottom only 17 carats and half. (Reaumur's grain is a twelfth of a carat, a division used in Germany.)

Wicklow gold.

Mr. G. A. Deluc announced in the Journal de Physique, vol. LII, p. 205, that pieces of gold, found in the county of Wicklow, in Ireland, contained a ninth of their weight of silver, without any other alloy.

Piece belonging to the academy.

My father, having been appointed to assay the piece of native gold belonging to the academy, during the time of the revolution, made two assays of it, both of which were 23 car. 26 thirty-seconds. This comes very near to pure gold; and proves, that gold is found in nature alloyed with very variable quantities of silver.

Pure native gold.

Mr. Fabbroni is the first who has demonstrated, that gold is found also quite pure. This is an important observation; but it does not seem to me to overturn the general principle, that native gold is a natural alloy of gold and silver: a principle established by a great number of facts, and to which only one exception is yet known.

Presence of lead in ancient coins to be sought.

It is desirable, that the presence of lead should be sought for in ancient coins or medals: as this would be the most certain method of ascertaining, whether the ancients refined their gold, or employed it as nature gave it them.

VII.

VII.

A Rejoinder to a Paper published in the Philosophical Journal, by Dr. MARCET, *on the Animal Fluids. By* GEORGE PEARSON, *M. D. F. R. S., &c.*

To W. NICHOLSON, Esq.

SIR,

BY a severe accident I have been prevented from writing the paper, which I proposed in the communication honourably inserted in your Journal for February last. Meanwhile an answer has been published by Dr. Marcet*.

Before I redeem my pledge of offering some remarks on Dr. Marcet's Memoir, the subject of my former communication, I feel myself called upon by what I consider to be the true interests of science, to reply to his intervening answer. This gentleman cannot be more averse from polemical writing than I am, nor have more powerful motives of private advantage by being otherwise employed: but unless I were to avail myself of the plea of a celebrated philosopher, who asserted, that his regard for *truth* was so great, that he would not part with it, lest it should be ill treated by mankind, I have no option consistent with public duty. The feelings of either party must however regulate their future conduct. For myself I can only promise, that I shall not consider it as a point of honour to contend for the last word. Reply to Dr. Marcet.

In the answer, which has been addressed to me, Dr. Marcet has set forth evidence from his memoir, still under examination, to maintain, that soda in an uncombined state, and not potash, exists in the animal fluids, as I trust I have legitimately proved according to facts hitherto discovered. As my honourable Opponent has not contravened the most decisive parts of the evidence in support of my allegations, I am spared the pains of again displaying it; so that I have only to comment on the evidence he brings forward in justification. Whether the animal fluids contain soda, or potash, uncombined.

* See the Philos. Journal for March last.

fication. In my remarks perhaps I cannot entirely avoid repetition of objections already produced.

Figure of crystals not a decisive proof.

The first kind of proof, that soda and not potash is present, again asserted by my adversary, is from the figure of crystals. I have to remark in addition to my former observations, that their forms *alone*, rarely or never, even when perceivable with the unassisted organ of vision, do singly denote unequivocal properties: and when not perceivable without the medium of glasses, we know from past experience the figures are to be considered as still more equivocal, I might say deceptive. If these crystalline forms are now admitted as justly distinguishing properties of certain substances, it is in consequence of repeated observation on larger quantities by direct vision, "quæ sint oculis subjecta fidelibus"; but even then not without concomitant other well ascertained properties.

Acetic acid said to have formed acetate of soda.

Secondly, great dependence seems to be placed on the acetate produced by combining acetic acid with the saline matter afforded by incineration. This was said to be acetate of soda, which dissolved in alcohol, "while potash was found in the residue left undissolved by the alcohol". I have searched the pages of the memoir under examination, again and again, for the evidence in support of this allegation; but, here and on many other occasions, is a mere assertion, except a partial support from the serum of the blood, as will be seen hereafter. For 1st, with regard to the saline matter of the fluid of the *spina bifida*, I find these words, "the alcoholic solution being decanted off and evaporated to dryness, a residue *supposed to consist of acetate of soda* was obtained." Here no mention is made either of an experiment to prove whether the acetate was that of soda or of potash, but it was *supposed* to be acetate of soda. As to the undissolved matter containing potash, there is not even that I can find a word written. This too, has been *supposed*.

Fluid of spina bifida.

Fluid of hydrocephalus internus.

2. With regard to the second fluid examined, that of hydrocephalus internus, we are told, "the analysis was conducted in the same manner as in the former"—of course the existence of soda in the alcohol, and of potash undissolved, is not proved, but here also *supposed*.

3. In

3. In the examination of other animal fluids, viz. of ascites, of hydrothorax, and hydrops pericardii, as well as subsequently of the hydrocele, of the hydatids, of the thyroid gland, and of a tumour of the chest, no such experiment as that of compounding an acetate is mentioned. Other fluids.

4. In the experiments however on the saline matter of the serum of the blood, an acetate was compounded, which dissolved in alcohol; the words of the author being, "the alcoholic residue, *contrary to my expectations*, exhibited traces of potash, both by means of tartaric acid, and oximuriate of platina." This, as far as I can find, is the sole experiment with acetic acid and alcohol, related by the author to determine the kind of alkali present, although the assertion is made of the animal fluids generally. But, although the assertion be not proved, it may be worth while to consider what, or whether any thing is proved by these experiments. They prove, that potash was present, because there was a precipitate with tartaric acid, but nothing more—there is no proof, that it was in the state of muriate, as asserted. It perhaps will be said, that these experiments prove, that this "alcoholic residue" contains also acetate of soda; "for the same residue, treated with nitric acid, was almost entirely resolved into rhomboidal crystals, among which I was unable to detect any distinct prisms." Now I have already expressed my want of confidence in the figure of minute crystals *singly* as evidence, especially seen through glasses; and here I presume is a decisive instance of their fallacy; for the potash being proved to be present, and, as already said by Dr. Marcet, united to muriatic acid, it must have afforded cubes, if reliance can be placed on forms; but no such cubes were seen. A farther objection occurs to my mind in this experiment. I apprehend, that it is quite as likely to be true, that alcohol will dissolve a small proportion of muriate of soda, as according to Dr. Marcet it does of muriate of potash. This being the case, the "alcoholic residue" ought to have afforded cubes of muriate of soda as well as of muriate of potash. The process under examination requires farther animadversion on the remaining part of it; "Potash was easily discoverable in the residue insoluble in alcohol, which residue had now lost its deliquescent

Serum of blood. Experiment with acetic acid and alcohol.

What is proved by it?

deliquescent quality." That potash was present in a combined state I admit may be inferred, but I say confidently there is no proof, that it was united to muriatic acid. It is not however incumbent on me, but on the Affirmer, to show with what it is combined. I think it right to notice another unsatisfactory part of the process before me. It is said, a concentrated solution of the saline mass in question did not distinctly indicate potash by oximuriate of platina, but did by tartaric acid. Subsequently however we are told, that the dissoluble, as well as the indissoluble residue, of the acetous compound in alcohol readily denoted the presence of potash to the oximuriate of platina as well as to tartaric acid. To me I own this account only shows, that the quantities employed were too minute for distinct observation of facts. How all ambiguity might have been removed I have taken the liberty of proposing in commenting on this process in my former communication to your Journal, p. 151. On that occasion I expressed my doubt, whether or not the acetate of soda be dissoluble in alcohol, but I referred to the authority of experiment. Here, my learned friend exultingly construes these phrases of doubt, *two palpable errours*, and triumphs—"a hit, a hit, my Lord, a very palpable hit."—No, there is no errour in this case, Dr. M., according to the *English* meaning of the terms used. To make the most of these *asserted errours* I am also charged with no less than three times repeating them; as if the propriety of writing was absolutely limited to the number of times an assertion should be delivered. At this time however, without the slightest uneasy emotion, I say, that acetate of soda is a deliquescent salt, and dissoluble in alcohol; for I have performed the necessary experiment, not indeed with "half a grain and a watch glass," but with 50 grains. The truth is, I had not leisure, little as was required, when I wrote my communication, to make the experiment; but as, on inquiry of a friend most likely to be informed, I found he was ignorant; as on just looking into two valuable books, Aikin's Dictionary and Thompson's Elementary work, one said the acetate of soda was a permanent and the other a deliquescent salt; and as in my collection of specimens, there was a permanent crystallized salt

Another point not satisfactory.

Acetate of soda deliquescent, and soluble in alcohol.

salt labelled by an assistant acetate of soda; I thought it best to leave the matter as doubtful, although I own I inclined to the contrary opinion of that which is I now believe the truth. Dr. M. may call this a *palpable errour*, if he pleases—he will hurt nobody but himself by the phrase. The main proof is hereby not affected; for the fact now ascertained against my doubtful opinion is only a collateral evidence on either side.

Proof of combined potash none of soda.

5. Another source of evidence against me is that potash combined "was proved by the tests oximuriatic of platina, and tartaric acid." The just inference has been already proposed; but I will now remark that this experiment does not prove, that soda was or was not present.

Other insufficient evidence.

As to any other proofs they have been already minutely examined in my former communication, or have been answered in this: but I entreat the indulgence of being allowed to make two or three farther remarks. 1. On the fluid of the spina bifida, of the thorax, and of the pericardium, the tartaric acid was not employed at all. Of these fluids the analysis in general was very partial. 2. Of the alkaline matter of the hydrocephalus fluid the examination must be unsatisfactory by the tests, on account of the impracticability of entirely separating the two alkalies from one another in such minute quantities as were obtained; and, if the separation were not effected, as the two fixed alkalies are affirmed to exist, the test, tartaric acid, must have produced soda-tartrate of potash; consequently the inference of the adverse party cannot be just.

Having, as briefly as seemed proper, commented on the opposing evidence, and set forth in a different light my own, I must pay due respect to the other parts of the ingenious Answerer's paper.

Farther remarks.

If it shall appear, that the only difference in the results of the inquiries by the two parties, worth particular notice, is with respect to the alkaline matter, I submit to the world, whether or not Dr. M. could with prudence have published his memoir without a reference to his predecessor, as he observes he could have done with propriety; and especially as he says he was directed particularly to the alkaline impregnation by my paper. Dr. M. complains, that he is

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at a loss to understand my meaning, and is much embarrassed by my obscure and inaccurate manner of writing. I am grieved, that my learned friend should experience these difficulties; but as I have not heard similar complaints from others, I may perhaps not indecorously venture to say, that I suspect his claim to judgment of propriety and perspicuity in English is somewhat doubtful.

Whether substances be more distinguishable in large bodies, or in small.

My ingenious Opponent cannot agree with me, that substances and properties of substances are discoverable by operating upon large masses, which cannot be effected with smaller quantities. I really thought the proposition so obviously true, that illustration was needless. Heaps of illustrative examples in nature occur to my mind, while I am writing, both in the department of chemistry and physiology. If arsenous acid, muriate of soda, or sulphuric acid, be dissolved in the proportion of one part to 100 equal parts of water, they will be discoverable by well known reagents; but if the proportion of water be increased more and more, the indication of their presence will become less and less distinct, and at last they will be no longer perceivable, although it is known they exist: or if I take certain fractional designated parts of any given weight of these substances, they will elude manifestation by any means hitherto known. On this principle of division and diffusion the most deleterious poisons become innoxious by the minuteness of the quantity applied to the human constitution. Hence atmospheric air containing fen miasmata, plague contagion, or small pox matter, are applied with impunity. A pound of blood of a glandered horse transfused into a healthful horse will not excite disease, but as much blood as can be transfused from two glandered horses into one horse will excite the disease of glanders. Sugar, alkali*, &c. may exist in the blood, but not be discoverable by any known reagent on account of the small proportion of them existing in the blood at any given time, as I humbly reason,

* In Dr. Rollo's work on diabetes I have related an experiment, in which potash was taken in such quantity, that the urine became so impregnated as to afford a precipitate of super-tartrate with tartaric acid, at the same time the blood did not indicate a trace of alkali; owing, as I concluded, to the small proportion of alkali to the blood.

and

and not on account of an hypothetical new channel, a sort of northwest passage from the stomach to the urinary bladder. In the case of waters the proportion is so minute of various impregnating substances, that, unless very large bulks be used, they must escape detection. The great masters have accordingly used such large quantities. Margraf, (Opuscules chymiques, v. II, p. 8) did not evaporate 100 drops of snow or rain water in a watch glass capsule like some modern microscopic chemists, but he operated upon 100 quart measures of snow water, in which he was able to find only 60 grains of carbonate of lime, a few grains of muriate of soda, and traces of nitrous acid.

I had the advantage of making my juvenile efforts to perform several chemical exercises under that great master, professor Black. Among other precepts, treasured in the tablet of my memory for more than 30 years, was that of employing large bulks of mineral waters; and of all other things, in which there was a probability of minute proportions being present. The reasons of Dr. Black for not practising according to this rule in the instance of the analysis mentioned I cannot pretend to assign; but it seems probable, that he was in possession of only a small quantity of the material. As to the magnitude of the masses of matter required, it is impossible to specify them; but it is obvious, that analysis must fail to develope substances on account of the minute proportion to other things with which mixed not being susceptible of being made evident to the senses; and in consequence, by a due larger proportion they may be rendered sensible. Hence perhaps, it is that we are ignorant of many of the properties of light, calorific, electricity, of infectious, and contagious substances, &c. It is argued against me, that "the chemical properties, which belong to a particle of matter, belong to the whole mountain of the same substance." True—but I know nothing of properties of substances but by means of the external organs of sense, (this is indeed an axiom) and unless the particle be of a due magnitude, my organs of sense cannot inform me of its properties.

My honourable adversary talks of the advantage of a small scale of operation in the points of economy and convenience. Granted— Advantages of a small scale of operations

denied in some respects, Granted—but these are minor considerations indeed to the acquirement of knowledge. When Dr. Marcet also speaks of the advantage in point of accuracy, I protest against it for reasons above explained. It is farther represented, that "there is a degree of *neatness* gained by reducing the scale of operations". I own I have difficulty to conceive a just sense in which this term may be used on this occasion. Does it mean the avoiding extraneous things occurring in operations? if so, I cannot separate it from *accuracy*; and as it is seldom practicable to operate without meeting with some extraneous matter or dirt, it appears to me, that many of those old chemists, who are reproached for mentioning "a little dirt in their results", are more accurate than those modern chemists, who make up a "neat" tabular exhibition of the constituents of substances in centesimal quantities, which they have never weighed; and even of which substances there is a palpable deficiency of proof. If by *neatness* be meant the instruments employed, it would be as injudicious to prefer neatness to knowledge, as euphony of style to perspicuity.

but admitted others. A proud list is displayed of discoveries achieved by microscopic experiments, or on small masses of matter; but that was needless. I never disallowed the utility of such experiments. My plain answer is this—that for certain purposes all the knowledge that is wanted is attainable, and most easily, by operations on the small scale—that such is the nature of our present instruments, that it is only practicable to work on small quantities of some kinds of matter—that on almost all occasions it is advantageous to commence an intended perfect investigation with experiments on small masses, in order to enable the mind to invent subsequent experiments, and perform decisive operations on larger

Superiority of experiments on a large scale. quantities. As to the successful practices referred to, they only manifest, that much may be accomplished with inferior means; but it is demonstrable, that the same persons could have attained infinitely more by superior instruments, and in the more favourable circumstances of adequate quantities. In chemistry, I consider illustration by examples to be superfluous. Physic furnishes new illustrations analogous to the questions under discussion. Sydenham, without

out chemistry, with seemingly little of anatomical and physiological knowledge, as well as of natural history, has meritedly acquired the credit of one of the greatest Improvers; if he could acquire so much without these auxiliaries, it appears according to all reason, that by means of them much more would have been achieved. I might however exemplify the advantages for which I am contending by the conduct of Dr. Marcet himself. It appears, that he performed the analysis of two animal fluids, of the component ingredients of which he has given an account to the one hundredth part of a grain, without finding potash in any state. Subsequently however this alkali was detected in other animal fluids, the author's attention being directed, as he is pleased to say, by my published paper on expectorated matter, and by my conversations. Whether otherwise Dr. Marcet would have found out the potash, let others determine. Notwithstanding the sneering remark of his ounce or two of dropsical liquid being in competition with my two or three pounds of "ropy sputum," I should be very unreasonable if I were not, after this practical proof of the inadequacy of his method, to be well contented. If however instead of treading the primrose path of the new microscopic chemical school, he had condescended and submitted to the task of labouring in the "large, dismal, subterraneous laboratory;" if, I say, he had been there employed instead of in dalliance at "the fireside of his comfortable study;" I am confident it did not require his talents, to have done much more than nearly confirm the results of my experiments on animal substances. If too I can see the future in the instant, it will be only by experiments on very large quantities of the animal fluids, that discoveries will be effected of more of their impregnating ingredients, on account of the very minute proportions in which they exist.

Why Dr. Wollaston was mentioned as concerned in the inquiry.

Dr. Marcet thinks it worth while to disclaim his memoir as the joint work of Dr. Wollaston and himself. I cannot have the smallest objection, indeed by this I gain strength to my side; for the demand of justice alone compelled me to consider this writing as I have done. I must however cite a passage for justification. Beside the advantages from Dr. Wollaston's writings and conversations Dr. Marcet

Marcet owns "his *kind personal assistance in this and other similar inquiri s.*"

Charge of misquotation, I am accused of the unwarrantable licence of "quoting in italics, and placing between inverted commas words which have not been used by my adverse friend." Such base proceedings I am charged withal! As for italics I knew no better than that all writers for the sake of emphasis do employ such letters either for their own words or those of other writers. The word *elegant* so complained of is not intended as a quotation, it is my own word, which Dr. M. misrepresents. As for inverted commas, the few passages which they include I think no one would apprehend are Dr. Marcet's writing, except in two or three instances. Here I cannot perceive any misquotation but in one place. There I confess my heinous offence, and express sincere contrition, viz. for "fireside of the *drawing room*," in future read "the large, dismal, subterraneous laboratory is now changed for the fireside of *a comfortable study*."

of irony, Again; my respectable adversary is, I find offended, with what he is pleased to construe *irony*. I can do no more than declare, whether I shall again be accused of irony, or not, that I entertained more of respect than sufficient for subduing any such humour.

of jocularity. Another offence is *jocularity*, not suitable for the advancement of science. If in such a vein I have offendingly written, "I have shot mine arrow o'er the house and hurt a brother." This mode of writing however has the high authority of a great poet, and still greater philosopher—

.... ridentem dicere verum
Quid vetat?

I wish I could more frequently be jocular, as so many occurrences are experienced in common life to make one sad. Hence I would rather live with Horace, than with the melancholy moralist Jaquez. Some allowance too should be made for the differing natures of individuals from the elements being so differently mixed up.

"Nature hath fram'd strange fellows in her time,
Some being of such vinegar aspect,
That they'll not show their teeth in way of smile,
Though Nestor swear the jest be laughable."

The

The foregoing pages of rejoinder will, I trust, save me the trouble of many intended remarks on Dr. Marcet's paper, independently of its relation to the questions at issue. A few comments only I shall now beg to be allowed to deliver.

Substances found in animal fluids.

1. The *animal matters* in the fluids examined are stated to be of *two kinds*: viz. coagulable or albuminous matter, and what the author calls *muco-extractive*. I do not at all object to the experiments, but appeal to competent judges, whether it is not unjust to make this distinction. The evidence of the *coagulable* matter is from the visible coagulation by calorific, and some reagents, but if there be not a due proportion of it to the water in which it is dissolved, such evidence is not obtainable. This may be easily proved, and as I apprehend I have shown in my published papers, by a kind of synthetic experiment. For example: serum of blood, or any other known coagulable fluid, may be so diluted with water, as to afford no clear proof of its presence by coagulation on applying calorific, although such an effect may be reasonably inferred on probable grounds from the disturbance of transparency, or cloudiness. And, as far as I have found by experiment, coagulable matter so diffused, on being collected by evaporation to dryness, is scarcely coagulable by calorific; so that the whole of any given quantity of animal coagulable fluid by such treatment was rendered uncoagulable. According to my trials too, there always remained, on coagulating serum and other analogous fluids, a small proportion of animal matter dissolved in the watery part, which differed in no respect from the matter left on evaporating water containing a certain small and uncoagulable proportion of serum added to the water as above stated. But these dilute solutions, which appear uncoagulable, denote the presence of animal matter to the test of tannin. It was probably this property, and the animal matter afforded by evaporation, which induced some chemists to conclude, that a different kind of animal substance from coagulable, such as gelatinable, existed in the serum of blood. Hence I conclude, that the two grains of what Dr. Marcet calls *muco-extractive matter*, afforded by 500 grains of serum, after separating 44 grains of albumen or coagulable matter, is this matter rendered uncoagulable by dissolution. And hence too I conclude, that the animal

The animal matter in the fluids of one kind only.

animal matter, in the other animal fluids, which he examined, was of *one* kind only, viz. coagulable matter, but not demonstrable by its most distinguishing property on account of dissolution in a large proportion of water.

Ammonia not mentioned.

2. *Ammonia* is not mentioned among the impregnating ingredients. This is to me not surprising, for it is evidently from my experiments in so small a proportion as to be undiscoverable in the quantities employed. If I could not find by estimation half a grain weight of it in 7 or 8000 grains of animal matter, it was not likely to be rendered evident in 7 or 800 grains.

Sulphate of potash.

3. *Sulphate of potash.* That a *sulphate* exists I perceived evidence, and have accordingly inserted it among the saline matters in my published papers; but that it is *sulphate of potash* I apprehend will not be allowed to have been shown by Dr. Marcet.

Phosphate of lime, iron, and magnesia.

4. *Phosphates of lime, of iron, and of magnesia,* are enumerated in the memoir before me. Of *phosphate of lime* there is good evidence, as I have set forth, and coincide in my results with those of the author: as well as that there is probably sulphate of magnesia: also, that there is iron; but I was not able to infer, that it was in a state of phosphate, I only inserted it in my results as an oxide.

The colouring matter of the blood not iron.

Although it is not essentially connected, I take this opportunity of referring to a process, which I offer as evidence against the common opinion, that the colour of the blood is owing to iron. I have mentioned it in my lectures during several past years, and it was published in the Edinburgh Medical and Surgical Journal, vol. VII, p. 124, for January, 1811. I collected 110 grains of the red part in a dried state, by repeated ablutions from about 10000 grains, or upwards of twenty ounces of blood. By burning in a platina crucible, it afforded, in weight, two grains and a half of a half-fused brown tasteless substance. By boiling in muriatic acid a part was dissolved. This solution was not styptic to the taste; it became blackish on adding tincture of gall nut, and on adding prussiate of potash it afforded a deep blue coloured precipitate, which did not yield by ignition, or calcination, above half a grain of reddish brown powder. Is it then probable, that twenty ounces of blood should derive their colour from half a grain of

of oxide of iron? I think proper to speak of this result at this time because it was published anonymously, and because subsequently to its publication I find it has been mentioned by other persons without acknowledgement, or at least without knowledge of this fact.

5. I found also indications of *caronate of lime* and of *silica*, not enumerated by Dr. Marcet. Future experiments must furnish unequivocal evidence. Silex, and carbonate of lime.

6. *Muriate of potash* asserted by the author, instead of *potash* united to animal matter, or to some other destructible substance, as I have inferred. On this question perhaps more than necessary has been already said in the present and former papers. Muriate of potash.

7. *Subcarbonate of soda*, asserted by the author, has been the subject of discussion at the same time as the last mentioned ingredient. Subcarbonate of soda.

8. *Muriate of soda*. Both parties agree in this being the chief saline impregnation. Muriate of soda.

It may be right to notice, that I have employed the term *self-coagulable lymph*, instead of the usual one *coagulable lymph*; because the serum, another fluid of the blood, is also *coagulable*, but not of itself without a certain temperature, or certain substances being mixed with it. The deposit spoken of by Dr. Marcet is not, I think, as he supposes, what I mean by the term self-coagulable lymph. Self-coagulable lymph.

Although, if the cause of truth require it, another communication may be offered; it will be most agreeable to me, that it be not found necessary. Considering the erroneous inferences, with which the writings of chemistry by men of the greatest celebrity abound; I shall on that account endeavour to find a source of consolation, if time show, that I am the erring party. I hope too, that this controversial discussion may serve to promulgate knowledge, by inducing some persons to attend to the subject, who might not otherwise have known the original papers. If with these reflections my respectable adversary can be satisfied, the controversy will now be terminated. Conclusion.

" Claudite jam rivos pueri: sat prata biberunt".

George Street, Hanover Square, G. P.
April the 17th, 1812.

VIII.

METEOROLOGICAL JOURNAL.

1812.	Wind	Pressure. Max.	Min.	Med.	Temperature. Max	Min.	Med.	Evap.	Rain	
3d Mo.										
March 6	S W	29·88	29·84	29·860	56	41	48·5	—		☽
7	N W	29·87	29·66	29·765	57	35	46·0	·12	0·18	
8	N W	30·19	29·87	30·030	50	31	40·5	—	3	
9	N E	30·26	30·19	30·225	46	33	39·5	—		
10	N E	30·26	30·20	30·230	44	30	37·0	—		
11	N E	30·20	30·20	30·200	46	33	39·5	—		
12	N W	30·20	29·96	30·080	44	34	39·0	—	0·12	
13	N E	29·99	29·96	29·975	45	34	39·5	—	6	●
14	N E	29·99	29·87	29·930	44	26	35·0	—	4	
15	N E	29·87	29·76	29·815	42	31	36·5	·48	1	
16	N E	29·77	29·75	29·760	35	31	33·0	—		
17	N E	29·75	29·66	29·705	36	29	32·5	—		
18	N E	29·66	29·40	29·530	39	26	32·5	—		
19	E	29·40	29·30	29·350	39	29	34·0	—	0·14	☾
20	S W	29·24	29·10	29·170	50	40	45·0	—	8	
21	S E	29·54	29·24	29·390	54	39	46·5	·30	0·18	
22	N E	29·74	29·54	29·640	53	39	46·0	—	2	
23	S E	29·74	29·27	29·505	42	40	41·0	—	0·67	
24	N W	29·64	29·27	29·455	40	32	36·0	—	0·16	
25	N E	30.27	29·64	29·955	42	24	33·0	—	1	
26	S E	30·35	30·17	30·260	46	30	38·0	—		
27	S E	30·20	29·46	29·830	51	41	46·0	·36	0·16	○
28	S	29·42	29·25	29·335	53	49	51·0	—	—	
29	S W	29·48	29·36	29·420	58	48	53·0	—	0·46	
30	S W	29·78	29·48	29·630	59	40	49·5	·30	0·12	
31	E	29·53	29·48	29·505	47	40	43·5	—	0·10	
4th Mo.										
April 1	Var.	29·64	29·59	29·615	58	41	49·5	·18	—	
2		29·70	29·58	29·640	—	—	—	—	—	
3		29·68	29·58	29·630	55	43	49·0	·17	0·26	
		30·35	29·10	29·739	59	24	41·5	1·91	2·80	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES

NOTES.

Third Month.—9. A shower of hail p. m. 11. Hoar frost. 15. Frosty morning. 16. Wind very strong from N. E. all day. 17. Cold wind. 20. Snow in the morning, followed by rain. 22. Very wet night: high wind. 25. Snow: the barometer rising rapidly. 26. a. m. Very fine: barometer still rising. 27. a. m. Cloudy; a considerable depression of the barometer, with appearances indicating thunder. Late at night a shower of hail, with lightning. 28. Stormy, with showers. 29. a. m. Windy. At 2 h. 30 m. p. m., the temperature without being 54°, I found the vapour point in a room as high as 51°. In an hour after this it began to rain steadily, and there fell near half an inch depth. 30. Much wind, at intervals changing to E. 31. Stormy from E. and S. E.: cloudy: about 9. p. m. an extensive appearance of light in the clouds to the W. with rapid coruscations passing through them, in the manner of an aurora borealis. This phenomenon was apparently not more elevated than the clouds which then overspread the sky, and was certainly not produced by the reflection of a light situate below them: it continued 20 or 30 minutes.

RESULTS.

Prevailing winds easterly.

Barometer: highest observation 30·35 inches; lowest 29·10 inches;
Mean of the period 29·739 inches.

Thermometer: highest observation 59°; lowest 24°;
Mean of the period 41·5°.

Evaporation 1·91 inches. Rain, &c. 2·80 inches.

This, as well as the preceding lunar period, has been unusually productive of rain: the two afforded *six inches and a half* in fifty-nine days.

LONDON,
Fourth Month, 22, 1812.

L. HOWARD.

IX.

A chemical Account of an Aluminous Chalybeate Spring in the Isle of Wight. By ALEXANDER MARCET, *M. D., F. R. S., one of the Physicians to Guy's Hospital, and Member of the Geological Society*.*

Analysis of mineral waters.

THE accurate analysis of a Mineral Water, although attended with considerable difficulty and labour, must be allowed, in a general point of view, to be an object of so little importance, that unless there be some interesting medical question to investigate, or some new analytical methods to point out in the course of the inquiry, it may be questioned whether researches of this kind are worth the time and attention which they require, or deserve to be placed amongst the records of natural science.

Importance of the present subject.

Having thought it necessary, in the present essay, to confine myself to the natural and chemical history of the spring in question, without any digression upon its medicinal qualities, and being well aware, that chemical details are considered by geologists merely as collateral subjects, some apology may be required for the length of this communication. But if the relation which the history of mineral waters bears to geological and mineralogical inquiries, and the peculiarities of composition, for which this spring is remarkable, entitle the subject to the attention of this society, I hope, that the general views and investigations which I have occasionally introduced respecting the analysis of mineral waters, and the composition of several salts connected with this inquiry, will be deemed a sufficient excuse for having thus expanded an account, from which they were almost inseparable.

Inducements to the analysis.

It is about two years since my attention was directed to this chalybeate spring by Dr. Saunders, to whom, in consequence of his valuable treatise on mineral waters, inquiries of this kind are frequently referred. Having been requested by him, and soon afterward by the discoverer of the spring, Mr. Waterworth, surgeon, of Newport, to examine this water, I

* Transactions of the Geological Society, vol. I, p. 213.

soon

soon perceived by a few preliminary experiments, that its principal ingredients were sulphate of iron and sulphate of alumine, and that it possessed a degree of strength far more considerable than any mineral water of the same kind that ever came to my knowledge.

This last circumstance, and the probability that this spring might some day attract public notice from its medicinal properties, induced me to undertake the present analysis, which, after many interruptions, I have at length brought to a conclusion.

SECT. I. *Situation and Natural History of the Spring.*

This spring is situate on the south-west coast of the Isle of Wight, about two miles to the westward of Niton*, in one of those romantic spots for which that coast is so remarkable. Situation and natural history of the spring.

In its present state it may be said to be of difficult access, for there is no carriage road, nor even any regular foot path along the cliff leading to it, and the walk would appear somewhat arduous to those unaccustomed to pedestrian excursions. But it would be practicable, and probably not very expensive, to render this path equally easy and agreeable. It was in walking along the shore, a few years ago, that Mr. Waterworth's attention was accidentally directed to this spring, which he traced to its present source, by observing black stains formed by rivulets flowing from that spot.

With regard to the mineralogical history of that district, I have been favoured through the kindness of my friend Dr. Berger, who visited the spot very lately, with so much more accurate an account of it than I should, from my own observation, have been able to offer, that I shall make no apology for transcribing it in his own words. Mineralogy of the district.

"The aluminous chalybeate spring", says Dr. Berger, "issues from the cliff on the S. S. W. coast of the Isle of "Wight, below St. Catharine's seamark, in the parish "of Chale. The bearing of the Needles from the spot is

* On an Estate belonging to Michael Hoy, Esq.

"N. W.

Situation and natural history of the spring.

" N. W. while that of Rockenend, not far distant, is S. E. " by S.

" The elevation of this spot, as far as I could ascertain " it by the barometer, is one hundred and thirty feet above " the level of the sea. Its distance from the shore may be " about one hundred and fifty yards.

" The water is received into a basin formed in the rock " for this purpose, and flows, as I was informed, at the " rate of two or three hogsheads in a day. Its temperature " I found to be 51°, that of the atmosphere being 48°; " and it may be worth while to observe, that this tempera- " ture corresponds with that of several springs of pure wa- " ter which I have met with in the island.

" The lower part of the cliff is rather encumbered with " masses of rock, or portions of soil, which have fallen " from the upper strata. Immediately above these, the " spring issues from a bed of loose quartzose sandstone " containing oxide of iron. This sand, in which vestiges " of vegetable matter are discoverable*, alternates with a " purplish argillaceous slate of a fine grain, disposed in " thin layers, with a few specks of silvery mica, interspersed " through the mass. Black stains or impressions of vege- " tables are seen on the natural joints of this rock. Above " this lies a stratum, several fathoms in thickness, of a " blueish calcareous marl, with specks of mica, which has " an earthy and friable texture, and contains imbedded " nodules or kidneys of sulphuret of iron. Many of these " nodules have undergone a partial decomposition, to which, " no doubt, the existence of the principal ingredients of " the spring is to be ascribed. The upper strata of the " cliff are composed of a calcareous freestone, alternating " with a coarse shelly limestone, accompanied by nodules " or layers of *chert* or flint.

* On being sprinkled on a heated shovel, this sand scintillates as if undergoing a partial combustion. When submitted to chemical analysis, it yields a quantity of iron, but no lime, nor alumine, nor any other earthy matter soluble in an acid. Close to the spring this sand contains some traces of sulphuric acid, but not at a distance from it: it is evident therefore, that the sand rock is not the medium through which the spring is impregnated.

" As

"As the same arrangement of rocks here observed prevails in several other parts of the Isle of Wight, and even along the coast of Hampshire, it is not improbable, that other springs of a similar nature might be discovered. May not *Alum Bay*, which lies to the north of the Needles, have derived its name from a circumstance of this kind?

Perhaps similar springs in the neighbourhood.

"On the road from Shorwell to Chale, the soil consists of a ferruginous sandstone, and chalybeate iridescent waters are to be seen in several places. To the east of Freshwater bay, not far from the place where the cliffs of chalk begin to make their appearance, there is a rivulet, the taste of which strongly indicates the presence of iron. At Blackgang Chine, a little to the N. W. of the aluminous chalybeate, is another ferruginous stream running to the sea. The rock there is a sort of decomposed ironstone under the form of balls. The sound compact ironstone, having the appearance of flat pebbles worn by the rolling of the sea, occurs not unfrequently along the shore.

Other chalybeate waters.

SECT. II. *General Qualities and specific Gravity of the Water.*

a. The water issues from the sand rock above described perfectly transparent, and it continues so for any length of time, provided it be collected immediately, and preserved in perfectly closed vessels; but if allowed to remain in contact with the air, or even if corked up after a temporary exposure to it, reddish flakes are soon deposited, which partly subside, and partly adhere to the inside of the vessel.

General qualities of the water.

b. It has no smell, except that which is common to all chalybeates, and this it possesses but in a very slight degree.

c. Its taste is intensely chalybeate, and, beside a considerable degree of astringency and harshness, it has the peculiar kind of sweetness, which sulphate of iron and sulphate of alumine are known to possess.

d. Its specific gravity somewhat varies in different specimens. In three different trials I obtained the following results:

1st

1st specimen	1008·3
2d specimen	1007·2
3d specimen	1006·9
	3022·4
which gives a mean specific gravity of	1007·5

SECT. III. *Preliminary Experiments on the effects of Reagents.*

Experiments on it with various tests.

A. Paper stained with litmus was distinctly reddened by the water.

B. Paper stained with Brazil-wood was changed to a deep purple.

C. When agitated in contact with the air, or repeatedly poured from one vessel into another, the water became turbid, and on standing deposited reddish flakes.

D. On applying heat to a portion of the water just uncorked, and boiling it *quickly*, till it was reduced to one half or even one third of its original bulk, no precipitation whatever took place; but on continuing the evaporation, a white feathery crystalline substance appeared on the surface of the fluid, and on pushing the process still further, a saline matter of a pale yellowish green colour appeared, which continued to increase till the whole was reduced to a dry yellowish mass. These were the phenomena observed with water recently uncorked; but when, previous to the evaporation, it had been for some time exposed to the air, or when the evaporation was conducted very slowly, an appearance of reddish flakes was the first circumstance observed.

E. The mineral acids produced no obvious change in the water.

F. Oxalic acid produced a slight yellowish tinge; but no immediate precipitation or turbidness.

G. Oxalate of ammonia, in small quantity, likewise produced a yellow colour, without precipitate: but on adding more of this test a white precipitate appeared.

H. Prussiate of potash and infusion of galls produced abundant precipitates, the one blue, and the other black or dark purple; and the colour of these precipitates was much paler

paler when the water had not previously been exposed to the atmosphere.

I. Alkaline solutions produced copious greenish flocculent precipitates, which became darker on standing in the air.

K. Nitrate of silver occasioned a dense, white, but not considerable precipitate.

L. Both muriate and nitrate of barytes occasioned copious white precipitates.

M. A piece of marble being boiled for some time in a few ounces of the water, the marble was found to have undergone no sensible loss of weight by this operation; but its surface had acquired a faint yellowish tinge.

N. A quantity of the water being evaporated to dryness, and a considerable degree of heat applied to the dry residue, a solution of this in water had the same effect of reddening litmus as before.

SECT. IV. *Inferences arising from these Effects.*

1. From experiment A, connected with experiments C, H, I, M and N, and from the circumstance of taste, and other general properties, it appeared highly probable, that the water contained sulphate of iron, and perhaps also sulphate of alumine, without any uncombined acid*. Inferences from these.

2. From experiments C and D, it appeared evident that iron and lime were contained in the water, and that their solvent was not carbonic acid†.

3. The experiments D and E concurred to show, that the water did not contain any sensible quantity of carbonates.

4. The experiments F and G afforded additional evidence of the presence of iron, and, while they showed the existence of lime in the water, seemed to indicate, that the quantity of this earth was not considerable.

* Solutions of sulphate of iron, and sulphate of alumine, though made from these salts in their crystallized state, have, like acids, the power of imparting a red colour to litmus.

† The reddish flakes mentioned in C and D, and in Sect. II, *a*, are uniformly found to be sub-sulphate of iron.

5. It

5. It appeared probable from experiment K, that the water contained a small quantity of muriatic acid.

6. The change produced in experiment B, on the infusion of Brazil-wood, appeared at first ambiguous; it could not be owing to the prevalence of an alkali or carbonated earth, since the water turned litmus red, and since the presence of carbonated earths had been disproved by other results. But having found by comparative trials, that solutions of sulphate of iron changed paper stained with infusions of Brazil-wood to a black, or at least intensely dark violet colour, and that solutions of alum turned it crimson; and observing that a mixture of these solutions produced a dark purple hue, the appearance in question was easily explained.

7. The result of experiment L indicated the presence of sulphuric acid.

8. Upon the whole, and from a review of the foregoing experiments, the substances which, at this early stage of the analysis, the water appeared most likely to contain, were *sulphate of iron*, *sulphate of alumine*, *sulphate of lime*, and a small quantity of *muriatic salts*. Some sulphate of magnesia, and some alkaline sulphates, might possibly be contained in the water, though their presence could not be satisfactorily ascertained by these preliminary experiments.

SECT. V. *Gaseous contents of the Water.*

Gaseous contents of the water.

A quantity of the water measuring ten cubic inches, being boiled briskly over mercury, the gas given out, together with the air contained in the apparatus, was received in a graduated tube; on admitting caustic alkali into the tube, one tenth of a cubic inch of gas was absorbed. It appears therefore, that one hundred cubic inches of the water contain one cubic inch of carbonic acid gas, which is equivalent to about three tenths of a cubic inch to each pint. The water was uncorked at the moment of being examined, but I had not an opportunity of ascertaining the quantity of gas.

SECT. VI. *Evaporation of the Water, and Estimation of the Quantity of solid Ingredients.*

Quantity of solid ingredi-

1. Sixteen ounces of the water by measure, being evaporated down to a soft mass over a lamp, and afterward desiccated

cated in a drying apparatus at the heat of 180°*, the solid mass weighed eighty-six grains. During the evaporation the same appearances were observed as have been already related (in Sect. III, D.) and the dry saline mass assumed a pale greenish colour. On standing in the air, it slightly deliquesced, and its colour became somewhat darker. This saline mass, though slowly evaporated, never assumed a distinct crystalline appearance. ents left by evaporation.

2. I have stated before (Sect. II, d.) that some difference prevailed in the specific gravity of the several specimens of the water which were examined. A similar want of uniformity was observed in regard to the quantity of solid ingredients, as will appear from the following statement†.

	Grains.	
The 1st specimen yielded	86·	In the pint of sixteen ounces.
2d		
3d	63·6	
4th	80·4	
5th	82·8	
6th	77·2	
7th	84· ‡	
8th	78·	
	644	

These eight results therefore give 80·5 grs dried at 180°, as the average quantity of solid ingredients in each pint of the water.

* This is the heat I have usually employed for desiccation, because it is that which is obtained by the water-bath which I use, and can scarcely be raised higher by that apparatus. By a heat of 180° however, I generally mean some intermediate point between 170° and 180°, for it is impossible to regulate the temperature with perfect accuracy.

† In the first of these trials, a whole pint was evaporated; but in the subsequent ones, the quantity of water was diminished to eight, six, and sometimes only four ounces, all of which, for the sake of uniformity, I have reduced in the table to the common standard of the pint.

‡ This specimen I brought myself from the spring; the others were sent me in sealed bottles from the Isle of Wight.

SECT.

SECT. VII. *Of the different Methods of Analysis applicable to the present Inquiry.*

Different methods of analysis.

In analysing a mineral water, two modes of proceeding occur from the very first. We may either evaporate the water first, and apply our reagents to the solid residue; or operate at once upon the water itself. The former plan is in general found expedient when the quantity of the solid contents of the water is small; but when, as in the present instance, the impregnation is considerable, it may be more convenient to adopt the latter method. But at all events, as the redissolution of the solid residue, when the first mode of proceeding is resorted to, generally requires the introduction of an acid, which may modify or complicate the process, it is always desirable, that both methods should be tried in succession, in order to obtain comparative results.

We may also, if necessary, precipitate from the same portion of the water the several ingredients which it contains, by applying to it in succession their respective reagents; or, if our supply be considerable, we may use a fresh portion of it for each successive operation, a mode of proceeding which is generally preferable. No difficulty being experienced during the present inquiry in regard to the supply of water, a variety of methods was tried, with the details of which I shall not trouble the Society: but in order to convey a general idea of them, and in hopes that a summary review of this kind may afford some assistance to chemical inquirers not yet accustomed to researches of this nature, I shall briefly enumerate the different plans which presented themselves at this period of the analysis, and it will be seen afterward how these plans were gradually modified.

Methods employed.

1st method.

1st method. To precipitate in succession from a known quantity of the water, the *iron* by prussiate of potash—the *lime* by oxalate of ammonia—the *alumine* and *magnesia* by caustic potash, which, by boiling, redissolves the alumine and leaves the magnesia untouched.

2d method.

2d method. To precipitate the *iron* and *earths* by subcarbonate of ammonia. To evaporate the remaining clear solution to dryness, and apply a red heat. To redissolve this

this saline residue, and evaporate the solution slowly, in order to discover any fixed *alkaline sulphate* or *muriate* which may exist in the water. To boil in caustic potash the precipitate containing the iron and earths, in order to separate the *alumine* and *silica*. To dissolve the remaining mass (supposed to contain iron, lime, and magnesia) in nitric acid, evaporate to dryness, and apply a red heat, in order to render the peroxide of iron thus formed insoluble in acid. To add to the mass, minutely pulverized, nitric or acetic acid, as either of these acids will only dissolve the *lime* and *magnesia*, which may be separately obtained by their respective reagents. And lastly, to ascertain the quantity of *oxide of iron*, supposed to have been left untouched by the acid.

3d method. To precipitate from another portion of water the iron, lime, alumine, and silica, by a solution of neutral carbonate of ammonia, which reagent retains the magnesia in solution. To boil the precipitate in caustic potash, which takes up the *alumine* and *silica*. To redissolve in muriatic acid the residue not taken up by potash, which consists of lime and iron—separate the *iron* by pure ammonia, and the *lime* by oxalate of ammonia*. Precipitate the *magnesia*† from the clear solution by an alkaline phosphate. 3d method.

4th method. To evaporate to dryness a known quantity of the water, and to boil the residue in caustic potash, which will dissolve the *alumine* and *silica*, both of which may be precipitated again by muriate of ammonia‡. Treat the residue, insoluble in potash and supposed to contain *iron*, *lime* and *magnesia*, in the manner pointed out in the 2d method. 4th method.

* It is necessary to precipitate the iron before the lime, whenever any considerable quantity of sulphate or muriate of iron is present. For oxalate of ammonia acts upon solutions of iron, as will be fully explained under the head of sulphate of lime.

† The magnesia might be equally, and perhaps more conveniently separated, by boiling a known quantity of the solid residue in the neutral carbonate of ammonia, instead of applying this reagent to the water itself.

‡ The mode in which the silica may be separated from the alumine will be detailed in a subsequent part of this paper.

5th

5th method.

5th method. After having obtained by the preceding methods a knowledge of the proportions of iron and earthy substances, and formed an estimate of the nature and quantities of acids with which they are united, to ascertain in a direct manner the quantities of acids by their respective reagents, with a view to obtain a confirmation of the preceding results.

6th method.

6th method. To boil a known quantity of the water in succinate of ammonia, till all the *iron* and *alumine* are precipitated—edulcorate, precipitate and separate the alumine from the iron by boiling in caustic potash. From the clear concentrated fluid, to separate the *lime* by oxalate of ammonia, and the *magnesia* by pure ammonia; to evaporate the remaining clear fluid to dryness, and to apply a red heat, in order to burn or volatilize any remaining portions of the tests used in the processes above described. To redissolve the residue in order to ascertain by subsequent evaporation the presence and quantity of sulphate and muriate of soda*.

7th method.

7th method. To boil a known quantity of residue of the water in alcohol, in order to ascertain what salts it may contain, which are soluble in this menstruum.

Although I found it expedient, before advancing farther in the examination of the water, and in order to regulate my steps in the progress of the inquiry, thus to trace the various plans which seemed adapted to the purpose, yet I apprehend it would be superfluous to detail here in regular succession all the trials, which arose from these different methods. I shall therefore confine myself to such as belong more immediately to my object; and in relating them, shall consider singly, and under separate heads, the various ingredients of the water, stating, as I proceed, the proportions in which they were ultimately obtained.

Sect. VIII. *Sulphate of iron.*

Prussiate of potash does not ascertain the quantity of iron.

The presence of iron, in the state of sulphate, having been abundantly proved by the preliminary experiments, the next step was, to ascertain the proportion of this salt in a given quantity of the water. The first reagent which I tried

* This process is liable to an objection, which will be hereafter fully stated, namely, that muriate of soda is decomposed by sulphate of ammonia at a high temperature.

for

for this purpose was prussiate of potash; but after many trials (which afforded uncertain and discordant results, I convinced myself, that this test, however useful for detecting the *presence* of iron, is quite inappropriate when our object is to ascertain the *quantity* of that substance*.

Fifty grains of residue† dried at the temperature of between 170 and 180, (as described in sect. VI,) and therefore equal to ten ounces of the water, were boiled in successive solutions of the potash, so as to saturate all the acid contained in that residue, and to dissolve the alumine. The remaining solid residue, which had passed first to a dark green, and some hours afterward to a dark brown or nearly black colour, was dissolved in nitric acid, and the solution evaporated to dryness, after which a red heat was applied, in order to bring the iron to a state of peroxide, and thus render it insoluble in the same acid. The mass being now treated with nitric acid, in order to separate the lime and magnesia supposed to be mixed with the oxide of iron, and the whole being thrown into a filter, the clear solution was found still to contain a good deal of iron. This last solution was, like the former, evaporated to dryness, and to the resi-

Residuum of the water boiled with potash,

treated with nitric acid

* Prussiate of potash, as a precipitant of iron, is liable to the following objections :—

Objections to prussiate of potash.

1st. It is apt, although apparently well prepared and crystallized, to precipitate certain earthy substances, and in particular alumine; this I found distinctly to happen in two experiments, in which the mixture was heated.

2dly. If the solutions be used cold, and if the metal be not highly oxidated, some of the Prussian blue unavoidably passes through the filters; or if no filters be used, it subsides but slowly and imperfectly.

3dly. If the solutions be heated, the prussiate of potash is itself decomposed, and yields a quantity of oxide of iron, which vitiates the results.

† By the word *residue*, thus generally used, is always meant the residue of the water under examination, dried at the temperature of between 170° and 180°. And in comparing a quantity of residue with a corresponding portion of the water, the average proportion of 80·5 grs for each pint (sect. VI, 2) is always assumed as the standard of comparison.

due,

and with acetic, due, again heated to redness, acetic*, instead of nitric acid, was this time added, and the solution filtered. The filtered fluid still contained a quantity of iron, which, however, from subsequent examination appeared very inconsiderable.

and the iron reduced. The oxide of iron left in the filter being roasted with wax and heated to redness, in order to bring it to a uniform state of oxidation, weighed 6·8 grains†.

Residuum treated with carbonate of ammonia, 2. With a view to repeat and vary the last experiment, another portion of residue, also weighing 50 grains, was thrown into a solution of neutral carbonate of ammonia, the quantity of the latter being more than sufficient to saturate any acid present, and to dissolve the magnesia suspected to exist in that residue. A considerable effervescence took place. The mixture, after this, was gently heated and filtered. The residue left in the filter was of a

the filtered matter treated with potash, pale yellowish brown colour. The clear solution deposited a small quantity of precipitate similar to the residue left in the filter, to which residue this precipitate was added. The contents of the filter were then treated with potash, in the manner before described (sect. VIII, 1.), in order to sepa-

* The acetic acid, as well as the nitric, is said to be incapable of dissolving any iron, which has been peroxidated by the process just described. In this instance a few particles of oxide were taken up by the acid: but it is probable, that if, instead of heating the residue to redness only a few minutes, the oxide had continued exposed to a red heat for half an hour or more, the whole of it would have become insoluble.

State of the oxide. † It may be asked, in what state of oxidation the iron is after this operation? It has generally been supposed to be reduced to the state of protoxide in consequence of the affinity of the combustible matter for oxigen; but in an experiment, which I made some years ago to ascertain this point, (the particulars of which may be seen in my account of the Brighton chalybeate) this process appeared to bring the iron to the state of peroxide; for 100 parts of iron gave 147·6 parts of oxide, proportions which are now considered as constituting the red oxide of iron. And as a confirmation of this, I observe, that Dr. Thomson, in his valuable paper on the oxides of iron, published in the twenty-seventh volume of Nicholson's Journal, states (p. 379) that some of the red oxide being mixed with oil and heated to redness, till it became black and magnetic, no diminution of weight took place. Indeed I have always obtained by this process, not a black, but a brown oxide, which in cooling passes to a red brown colour, somewhat varying in shade, but mostly resembling powdered cinnamon, and being more or less magnetic.

rate

rate the alumine, after which the residue, now supposed to contain nothing but carbonate of lime and iron, was treated with dilute muriatic acid, which dissolved it with effervescence. From this solution the lime was precipitated by oxalate of ammonia, and the remaining liquor, now containing nothing but muriate of iron, was treated with carbonate of ammonia, so as to precipitate the whole of the iron, which, in subsiding, assumed a pale reddish colour. The clear fluid being decanted off, and the precipitate carefully washed, dried, and ultimately heated to redness with a little wax in a platina crucible, weighed 7·2 grs.

dissolved in muriatic acid, precipitated by oxalate of ammonia and the iron thrown down by carbonate of ammonia.

3. It will be observed, that between this and the former result there was a difference of 0·4 grs in the quantity of oxide of iron contained in 50 grs of residue. But when it is considered, that in the first of these analyses a small quantity of iron was positively detected in the acetic solution, which, from the best estimate I could make, would have brought the quantity of iron very near that obtained in the second process, it will readily be admitted, that the coincidence was such as to authorise me to consider the last result as sufficiently accurate*.

Difference of results.

4. If therefore we consider 7·2 grs of peroxide of iron, as the quantity of this metal contained in 50 grs of the residue, which corresponds to 11·59 grs of the oxide for 80·5 grs of residue (that is for each pint of the water according to the average before established, sect. VII, 2), we shall be able so infer the quantity of sulphate of iron contained in the water.

Proportion of oxide in sulphate of iron.

5. In order to do this, however, it was necessary to ascertain by a comparative experiment the proportion of oxide, which a known quantity of sulphate of iron yields by a process similar to that which I have just described. For

Proportion of oxide in sulphate of iron.

* In one experiment in which the iron was precipitated from a similar quantity of residue, by prussiate of potash, and the prussiate of iron roasted with wax, the quantity of oxide obtained amounted to 11 grs, from which I infer, either, that a portion of the oxide of iron, always contained in prussiate of potash, must have been precipitated with the Prussian blue, or, that the prussiate of iron was not completely decomposed in the process in question, or that some earthy substance was precipitated along with the iron.

this purpose, 50 grs of transparent crystallized green sulphate of iron were dissolved in water, and treated with carbonate of ammonia as long as any precipitate appeared. This precipitate, after being carefully separated, edulcorated, dried, and ultimately heated to redness with wax in a platina crucible, weighed exactly 14 grs. It appeared in the form of a red brown magnetic powder*.

Proportion of sulphate of iron in the water.

6. Since therefore 50 grs of crystallized green sulphate of iron gave 14 grs of this oxide, the 7·2 grs of oxide obtained from 50 grs of residue, would represent 25·7 grs of green sulphate of iron; and 11·59 grs of oxide (which is the quantity contained in an English pint of the water), would represent 41·4 grs of that salt.

(To be concluded in our next.)

X.

Experiments to ascertain the State in which Spirit exists in fermented Liquors: with a Table exhibiting the relative Proportion of pure Alcohol contained in several Kinds of Wine and some other Liquors. By WILLIAM THOMAS BRANDE, *Esq. F. R. S.*†

SECT. I. IT has been a commonly received opinion, that the alcohol obtained by the distillation of wine does not exist ready formed in the liquor: but that it is principally a product of the operation, arising out of a new arrangement of its ultimate elements.

Principal proof.

The proofs which have been brought forward in support of this theory are chiefly founded on the researches of Fabroni

* This result, which was obtained in two different trials, with the variation of only 0·1 gr. corresponds exactly with the proportions given by Mr. Kirwan, in his Treatise on Mineral Waters (table iv.), in which 28 grs are the quantity of oxide stated to exist in 100 grs of green sulphate. But, in order to establish the perfect coincidence of these results, it would be necessary to know the process which Mr. Kirwan followed. The iron in his experiment is stated to have been obtained in the state of black oxide.

† Phil. Trans. for 1811, p. 337.

Fabroni‡, who attempted to separate alcohol by saturating the wine with dry subcarbonate of potash, but did not succeed, although by the same means he could detect very minute portions of alcohol, which had been purposely added.

Brandy commonly added to wines.

To obtain satisfactory results from many of the following experiments, it became necessary to employ wines to which little or no spirit had been added; for very considerable addition of brandy is made to most of the common wines, even before they are imported into this country. I therefore occasionally used Burgundy, Hermitage, Cote Roti, Champagne, Frontignac, and some other French wines; to which, when of the best quality, no spirit can be added, as even the smallest proportion impairs the delicacy of their flavour, and is consequently readily detected by those who are accustomed to taste them. For these, and for the opportunity of examining many of the scarce wines enumerated in the table annexed to this paper, I am indebted to the liberality of the Right Honourable Sir Joseph Banks.

Good French wine would be spoiled by it.

Port procured purposely without it.

Dr. Baillie, who took considerable interest in this investigation, was also kind enough to procure for me some port wine, sent from Portugal for the express purpose of ascertaining how long it would remain sound, without any addition whatever of spirit having been made to it.

Raisin Wine.

Lastly, I employed raisin wine, which had been fermented without the addition of spirit.

Insufficiency of subcarbonate of potash to separate the alcohol.

At a very early period of the present inquiry, I ascertained by the following experiments, that the separation of the alcohol by means of subcarbonate of potash was interfered with, and often wholly prevented by some of the other ingredients of the wine.

Port wine distilled, and the spirit separated,

A pint of port wine was put into a retort placed in a sand heat, and eight fluid ounces were distilled over, which, by saturation with dry subcarbonate of potash, afforded about three fluid ounces of tolerably pure spirit floating on the surface.

but it could not when mixed with the residuum.

I repeated this distillation precisely under the same circumstances, and mixed the distilled liquor with the residuum in the retort, conceiving, that, if the spirit were a product, I now should have no difficulty in separating it from

‡ Ann. de Chim. vol. XXXI, p. 303.

from the wine by the addition of subcarbonate of potash; but, although every precaution was taken, no spirit separated; a portion of the subcarbonate, in combination with some of the ingredients of the wine, formed a gelatinous compound, and thus prevented the appearance of the alcohol.

Fabroni's experiments did not succeed with the author.

It has been remarked by Fabroni, in the Memoir above quoted, that one hundredth part of alcohol purposely added to wine may be separated by subcarbonate of potash; but several repetitions of the experiment have not enabled me to verify this result: when however a considerable addition of alcohol has been made to the wine, a part of it may be again obtained by saturation with the subcarbonate. The necessary addition of spirit to port wine, for this purpose, will be seen by the following experiments.

Subcarbonate of potash added to Port,

Four ounces of dry and warm subcarbonate of potash were added to eight fluid ounces of port wine, which was previously ascertained to afford by distillization 20 per cent of alcohol (by measure) of the specific gravity of 0·825 at 60°.

and the alcohol remained mixed with extract, &c.

In twenty-four hours the mixture had separated into two distinct portions; at the bottom of the vessel was a strong solution of the subcarbonate, upon which floated a gelatinous substance, of such consistency as to prevent the escape of the liquor beneath when the vessel was inverted, and which appeared to contain the alcohol of the wine, with the principal part of the extract, tan, and colouring matter, some of the subcarbonate, and a portion of water; but as these experiments relate chiefly to the spirit contained in wine, the other ingredients were not minutely examined.

One part of alcohol added to seven of wine, none could be separated.

To seven fluid ounces of the same wine, I added one fluid ounce of alcohol (specific gravity 0·825), and the same quantity of the subcarbonate of potash as in the last experiment; but after twenty-four hours had elapsed, no distinct separation of the alcohol had taken place.

One part of alcohol to three of wine: part separated.

When two fluid ounces of alcohol were added to six fluid ounces of the wine, and the mixture allowed to remain undisturbed for the same length of time as in the former experiments, a stratum of impure alcohol, of about a quarter of an inch in thickness, separated on the surface.

Three parts of alcohol to five wine.

The addition of three fluid ounces of the alcohol to five fluid ounces of the wine, formed a mixture from which a

quantity

quantity of spirit readily separated on the surface, when the subcarbonate was added, and the gelatinous compound sunk nearly to the bottom of the vessel, there being below it a strong solution of the subcarbonate.

When in these experiments Madeira and Sherry were employed instead of Port wine, the results were nearly similar. Madeira and Sherry.

It was suggested to me by Dr. Wollaston, that, if the wine were previously deprived of its acid, the subsequent separation of the alcohol, by means of potash, might be less interfered with. I therefore added, to eight fluid ounces of port wine, a sufficient quantity of carbonate of lime to saturate the acid, and separated the insoluble compounds produced by means of a filter. The addition of potash rendered the filtered liquor turbid, some soluble salt of lime, probably the malate, having passed through the paper; but the separation of alcohol was as indistinct, as in the experiments just related. Previous separation of the acid made no difference.

It is commonly stated, that the addition of lime water to wine not only forms insoluble compounds with the acids, but also with the colouring matter, and that these ingredients may be thus separated without heat; but on repeating these experiments, they did not succeed, nor could I devise any mode of perfectly separating the acids, and the extractive and colouring matter (excepting by distillation), which did not interfere with the alcohol. Lime water will not separate the acids and colouring matter perfectly.

If the spirit afforded by the distillation of wine were a *product*, and not an *educt*, I conceived, that by performing the distillation at different temperatures, different proportions of spirit should be obtained. Whether difference of temperature in distillation affects the spirit.

The following are the experiments made to ascertain this point.

Four ounces of dried muriate of lime were dissolved in eight fluid ounces of the port wine employed in the former experiments: by this addition, the boiling point of the wine, which was 190° Fahrenheit, was raised to 200°. The solution was put into a retort placed in a sand heat, and was kept boiling until four fluid ounces had passed over into the receiver, the specific gravity of which was 0·96316 at 60° Fahrenheit.* Port distilled at 200°,

The

* It was supposed that in this experiment a small portion of muriate of

190°, The experiment was repeated with eight fluid ounces of the wine without any addition, and the same quantity was distilled over, as in the last experiment: its specific gravity at 60° Fahrenheit, was 0·96311.

in a waterbath, Eight fluid ounces of the wine were distilled in a water bath; when four fluid ounces had passed over, the heat was withdrawn. The specific gravity of the liquor in the receiver was 0·96320 at 60° Fahrenheit.

and at 180°. The same quantity of the wine, as in the last experiment, was distilled at a temperature not exceeding 180° Fahrenheit. This temperature was kept up from four to five hours, for five successive days, at the end of which period, four ounces having passed into the receiver, its specific gravity at 60° was ascertained to be 0·96314.

No difference apparently in the spirit. It may be concluded, from these results, that the proportion of alcohol is not influenced by the temperature at which wine is distilled, the variation of the specific gravities in the above experiments being even less than might have been expected, when the delicacy of the operation by which they are ascertained is considered.

Attempt to separate the spirit by freezing. I have repeatedly endeavoured to separate the spirit from wine, by subjecting it to low temperatures, with a view to freeze the aqueous part; but when the temperature is sufficiently reduced, the whole of the wine forms a spongy cake of ice.

The same with a compound of alcohol. In a mixture of one fluid ounce of alcohol with three of water, I dissolved the residuary matter, afforded by evaporating four fluid ounces of Port wine, and attempted to separate the alcohol from this artificial mixture by freezing; but a spongy cake of ice was produced as in the last experiment.

But wine may be made stronger by freezing. When the temperature is more gradually reduced, and when large quantities of wine are operated upon, the separation of alcohol succeeds to a certain extent, and the portion which first freezes is principally, if not entirely water; hence in some countries this method is employed to render wine strong.

of lime might have passed over into the receiver, but the distilled liquor did not afford the slightest traces of it, to the tests of oxalate of ammonia and nitrate of silver.

SECT

SECT. II. Having ascertained, that alcohol exists in wine ready formed, and that it is not produced during distillation, I employed this process to discover the relative proportion of alcohol contained in different wines. Relative proportion of alcohol in wines.

In the following experiments, the wine was distilled in glass retorts, and the escape of any uncondensed vapour was prevented by employing sufficiently capacious receivers, well luted, and kept cold during the experiment. Mode of conducting the experiments.

By a proper management of the heat towards the end of the process, I could distil over nearly the whole of the wine without burning the residuary matter: thus, from a pint of Port wine, of Madeira, of Sherry, &c., I distilled off from fifteen fluid ounces, to fifteen fluid ounces and a half: and from the same quantity of Malaga, and other wines containing much saccharine matter, I could readily distil from fourteen to fifteen fluid ounces.

In order to ascertain the proportion of alcohol with precision, pure water was added to the distilled wine, so as nearly to make up the original measure of the wine, a very small allowance being made for the space occupied by the solid ingredients of the wine, and for the inevitable loss during the experiments: thus, five fluid drachms and a half of distilled water were added to fifteen fluid ounces and a quarter of the liquor procured by the distillation of a pint of port wine, and in other cases nearly the same proportions were observed. This mixture of the distilled wine and water was immediately transferred into a well stopped phial, and having been thoroughly agitated, was allowed to remain at rest for some hours; its specific gravity (at the temperature of 60° Fahrenheit), was then very carefully ascertained, by weighing it in a bottle holding exactly one thousand grains of distilled water at the above temperature, and the proportion of alcohol per cent, *by measure*, was estimated by a reference to Mr. Gilpin's tables*, the specific gravity of the standard alcohol being 0·82500 at 60°.

As the most convenient mode of exhibiting the results of these numerous experiments, I have thrown them into the form of a table; in the first column the wine is specified; the second contains its specific gravity after distillation, as above

* Phil. Trans. 1794.

above described; and the third exhibits the proportion of the pure spirit, which every hundred parts of the wine contain. I have also inserted porter, ale, cider*, brandy, and some other spirituous liquors, for the convenience of comparing their strength with that of the wines.

Proportion of alcohol in various fermented liquors.

Wine.	Specific Gravity after Distillation.	Proportion of Alcohol, per Cent, by Measure.
Port	0·97616	21·40
Ditto	0·97532	22·30
Ditto	0·97430	23·39
Ditto	0·97400	23·71
Ditto	0·97346	24·29
Ditto	9·97200	25·83
Madeira	0·97810	19·34
Ditto	0·97616	21·40
Ditto	0·97380	23·93
Ditto	0·97333	24·42
Sherry	0·97913	18·25
Ditto	0·97862	18·79
Ditto	0·97765	19·81
Ditto	0·97700	19·83
Claret	0·98440	12·91
Ditto	0·98320	14·08
Ditto	0·98092	16·32
Calcavella	0·97920	18·10
Lisbon	0·97846	18·94
Malaga	0·98000	17·26
Bucellas	0·97890	18·49
Red Madeira	0·97899	18·40
Malmsey Madeira	0·98090	16·40
Marsala	0·97196	25·87
Ditto	0·98000	17·26
Red Champagne	0·98608	11·30
White Champagne	0·98450	12·80
Burgundy	0·98300	14·53
Ditto	0·98540	11·95

* The proportion of spirit, which may be obtained from these three liquors, is subject to considerable variation in different samples: the number given for each, in the table, is therefore the mean of several experiments, as it did not seem necessary to specify them separately.

White

Wine.	Specific Gravity after Distillation.	Proportion of Alcohol, per Cent, by Measure.
White Hermitage	0·97990	17·43
Red Hermitage	0·98495	12·32
Hock	0·98290	14·37
Ditto	0·98873	8·88
Vin de Grave	0·98450	12·80
Frontignac	0·98452	12·79
Cote Roti	0·98495	12·32
Rousillon	0·98005	17·26
Cape Madeira	0·97924	18·11
Cape Muschat	0·97913	18·25
Constantia	0·97770	19·75
Tent	0·98399	13·30
Sheraaz	0·98176	15·52
Syracuse	0·98200	15·28
Nice	0·98263	14·63
Tokay	0·98760	9·88
Raisin Wine	0·97205	25·77
Grape Wine	0·97925	18·11
Currant Wine	0·97696	20·55
Gooseberry Wine	0·98550	11·84
Elder Wine	0·98760	9·87
Cyder	0·98760	9.87
Perry	0·98760	9·87
Brown Stout	0·99116	6·80
Ale	0·98873	8·88
Brandy	0·93544	53·39
Rum	0·93494	53·68
Hollands	0·93855	51·60

Proportion of alcohol in various fermented liquors.

XI.

Meteorological Table for 1811. *In a Letter from the Right Hon.* W. J. LORD GRAY.

To WILLIAM NICHOLSON, Esq.

SIR,

THE inclosed Table is the result of last year's observations at Gordon Castle, the residence of the Duke of Gordon; which, if you choose, you may publish.

Meteorological table.

It

Improvement of the science. It would facilitate the promotion of meteorological science very much, if registers of the state of the atmosphere were more generally kept, and the results more publicly diffused.

I remain, sir,

Your most obedient humble servant,

GRAY.

TWICKENHAM,
17th of April, 1812.

Meteorological Table, extracted from the Register kept at Gordon Castle, County of Banff, N. Britain: Latitude 57 , 38. Above the Sea 100 feet.

1811	Morning, eight o'Clock. *Mean height of*		Aftn. three o'clock. *Mean height of*	Depth of Rain.	NUMBER of DAYS.			
	Barom.	Therm.	Therm.	In. 100	Rain or snow.	Fair.	West winds.	East winds.
January.	29·79	32·64	35·06	1·34	14	17	22	8
February.	29·34	34·57	38·18	2·65	14	14	14	14
March.	29·96	40·93	48·90	0·62	6	25	24	7
April.	29·71	42·90	48·30	3·93	21	9	14	15
May.	29·80	50·90	56·68	3·64	16	15	8	23
June.	29·87	54·93	59·33.	0·73	11	19	18	12
July.	29·98	57·90	61·55	2·09	17	14	22	8
August.	29·75	55·10	60·51.	4·03	21	10	26	5
September.	29·97	51·50	59·23.	2·53	11	19	28	2
October.	29·55	49·13	53·84	4·42	16	15	20	11
November.	29·71	42·36	44·83	2·30	20	10	27	3
December.	29·48	34·64	36·87	3·06	21	10	30	1
Average of the Year	29·74	45.62	50·27	31·34	188	177	253	109

N. B. The S. wind, and all to the W. of the meridian, are called west.

XII.

On Extract and the Saponaceous Principle; by Mr. SCHRADER, *of Berlin.**

AFTER quoting the works of Rose, Hermbstaedt, Trommsdorf, Fourcroy, and Vauquelin†, all of whom have examined these two matters, the author adds: Writers on the subject.

If oxidation be the principal characteristic of extract, cinchona is the substance that should be preferred for obtaining it. Accordingly I exhausted some cinchona (*china fusca et officinalis*) by alcohol, till the menstruum was no longer coloured. The tincture obtained had no action on solution of gelatine, but it reddened litmus paper, and precipitated sulphate of iron green. Peruvian bark preferred for obtaining it. Treated with alcohol,

Having subjected the tincture to distillation, water was poured on the residuum; when a sediment formed, which was separated.

The cinchona exhausted by alcohol was treated with cold water. Litmus paper and sulphate of iron were not perceptibly affected by the impression; but it precipitated gelatine. This aqueous solution was evaporated and redissolved several times, and each time the precipitate that formed was collected. Lastly, this extract was purified from *cinchonate of lime* by alcohol, and mixed with the alcoholic tincture. cold water,

The cinchona, that had been treated successively by alcohol and cold water, was boiled in water. The brown decoction was likewise evaporated and redissolved several times, taking care to separate the flocculent matter that subsided. This sediment afforded a brown powder, having distinctly the smell of extract of cinchona; which was little soluble in boiling water, or in alcohol, but formed a red and boiling water.

* Ann. de Chim. vol. LXXII, p. 290. Abridged from Gehlen's Journal by Mr. Vogel.

† See also a paper on vegetable astringents, by Dr. Bostock, Journal, vol XXIV, p 204, 241, in which the existence of extract as a separate principle is rendered very questionable. Dr. Henry however remarks, Elements of Chemistry, vol. II, p. 181, that Dr. Bostock did not examine the extract from saffron. C.

red liquor with caustic lixivium. When no more sediment formed, the liquor was added to the two preceding obtained by alcohol and cold water.

The extract oxided by repeated solution and evaporation.

More than a hundred evaporations and solutions were made by the help of a water-bath. Thus the pulverulent matter was reduced to a small quantity, and the colour of the liquid became deeper. By repeated solutions almost the whole was converted into powder. Of four ounces of cinchona, that had furnished the extract, 15 grains [?] of residuum were left, on which pure alcohol would not act.

Action of extract on iron,

The infusions of cinchona then contained extract, which precipitated iron of a green colour; a property that extract loses, when it reaches the maximum of oxidation.

gelatine,

Gelatine precipitates the extract of cinchona but in part, and the supernatant liquid comports itself in the same manner as infusion of cinchona.

and tin.

Solution of tin precipitates the infusion of cinchona, but the supernatant liquid has still the same properties; for it forms a green precipitate with iron, and, when boiled some time, the extract becomes oxided, and falls down in a flocculent sediment. Tin therefore precipitates the extract only in part; and the same takes place with lime water, or with a solution of alum.

Insoluble in alcohol.

Pure alcohol does not dissolve extract; and its action is still farther diminished by the oxidation of the latter.

Distilled water.

When cinchona, or its extract, is distilled with water, the product reddens litmus, without rendering the solution of iron turbid. But if extract of cinchona be distilled, till it becomes thick, the distilled product precipitates sulphate of iron green, and comports itself like the substance of coffee.

Attempt to obtain the saponaceous principle from gentian.

Properties of the matter obtained.

To obtain the saponaceous principle, the roots of gentian and of soap-wort were treated with alcohol. The alcoholic tincture of gentian was evaporated and redissolved in water. A resinous substance was deposited. The filtered liquor reddened infusion of litmus powerfully, but did not produce a green with solution of iron.

Neither muriate of tin, lime-water, nor gelatine, rendered the liquor turbid.

The liquid extracted by weak alcohol, and afterward diluted

diluted with water, containing the saponaceous principle, was evaporated and redissolved 40 or 50 times on a water-bath. Each time a brown powder, insoluble in water or alcohol, was thrown down.

The mother water of the infusion of gentian was oxided by oxigen gas, and by oximuriatic acid. The extract from gentian therefore is less greedy of oxigen than that from cinchona, but nevertheless it becomes oxided.

Though the infusion of gentian differs from that of cinchona in not acting on tin or lime, still we cannot say, that it contains a saponaceous principle.

If extract be insoluble both in pure alcohol and in ether; and the saponaceous principle, or the substance so called, enjoy the same properties; what are the characteristics of the latter?

Root of soapwort treated in the same manner.

The root of soapwort was treated in the same manner. The infusion comported itself with gelatine and the other reagents, like that of gentian.

The same with extract.

The saponaceous principle and extract, having the same properties, it should be called therefore, agreeably to the French chemists, extract.

Principle in coffee the same.

The matter in coffee announced as a new substance by Chenevix*, and by Payssé as a new acid†, does not differ perceptibly from the extract just described.

Properties of extract.

Extract then is an immediate principle of vegetables, existing under various modifications. It combines with several metallic oxides, particularly those of tin and iron, and produces a green colour with the latter. It unites also with lime, and with alumine. It always contains nitrogen. When concentrated it exhibits a transparent mass, more or less brown, which attracts the moisture of the air. Very frequently it contains free acetic acid, muriates, and a saccharine matter.

In living vegetables it appears to be colourless; but oxigen imparts to it a black colour. This appears probable from the sap of trees, which is white when it first flows from them.

Tannin proba-

It is very probable, that tannin is a modification of extract.

* See Journal, vol. II, p. 114. † Ibid. vol. XVII, p. 126.

It

bly a modification of it. It possesses all its properties, and in addition that of combining with glue.

General conclusions. Thus it follows, 1, That the saponaceous principle, which has been said to have been found in several vegetables, does not exist: it is nothing but extract.

2, That extract has the property of reddening the blue colour of litmus.

3, That this substance is soluble only in water and diluted alcohol; neither pure alcohol nor ether having any action on it when well dried.

4, That when it is diluted with a great deal of water, if it be boiled in contact with air it absorbs oxigen, and falls down in a powder insoluble both in water and in alcohol.

XIII.

An Examination of the Chromate of Iron of the Uralian Mountains, in Siberia: *by Mr.* LAUGIER*.

Chromate of iron found in France, IN the year 7 Mr. Pontier discovered in the department of the Var, near the mansion house of la Cassade, a mineral, which Mr. Tassaert first ascertained to be a compound of chromic acid, and oxide of iron. Mr. Vauquelin confirmed this analysis; and beside a difference in the proportions announced the presence of alumine and silex.

and in Siberia. Mr. Meder has since found in Siberia, in the Uralian Mountains, a substance much resembling the mineral of the Var. The examination of this substance, a specimen of which was presented to the author by Mr. Steinacher, of the Society of Apothecaries at Paris, is the subject of the present paper, in which the results of the author's analysis are compared with those of Mr. Vauquelin.

Comparison of the two. Though the Siberian mineral pretty closely resembles in appearance that from the Var, there is reason to conjecture, on examining it with attention, that the metal in it is more pure: its fracture, instead of being granular, is foliated; its metallic lustre is more vivid; and in general it is less

* Ann. de Chim. vol. LXXVIII, p. 70. Abridged from the Ann. du Muséum d'Hist. Nat. vol. VI, p. 325.

mixed

mixed with earthy matters. On some parts of its surface the specimen exhibits green spots, which may be known for oxide of crome. Its specific gravity supports the opinion of its greater purity. That of the specimen is 4·0579, while that of the mineral of the Var is only 4·0326. This difference in the gravity indicates of course a difference in the proportions of the metallic matter contained in these two varieties, and this the analysis proves.

Component parts of the Siberian mineral.

From the experiments related in the paper, which are too long to be inserted here, it follows, that the Siberian mineral contains, in 100 parts,

Oxide of chrome	53
——— iron	34
Alumine	11
Silex	1
	99.

and of the French.

These results differ a little in the proportions from the following obtained from the mineral of the Var by Mr. Vauquelin:

Chromic acid	43
Oxide of iron	34
Alumine	20
Silex	2
	99.

They are probably not chromates of iron, but compounds of the oxides of iron and of chrome.

Does chrome exist in the state of acid, or in that of oxide, in the mineral called chromate of iron? Mr. Godon de Saint-Mesmin, in a paper on the artificial combinations of chromic acid, is inclined to think, that it is in the state of oxide. Mr. Vauquelin, in his report on that paper, appears disposed to adopt the same opinion. Mr. Laugier agrees in it, and supports it by the following reflection. No direct experiment proves, that chrome is in the acid state in the native chromates of iron; and we have so much the less reason to think it, as slightly calcining the green oxide of chrome with caustic potash is sufficient, to convert it almost immediately into an acid: it is quite as probable therefore, that chrome is in the state of oxide in the minerals of the

Var

Var and of Siberia, as in that of acid; and quite as rational to consider them as compounds of the oxides of iron and of chrome, as chromates of iron.

The Siberian mineral analysed by Lowitz.

Since I have finished the examination of this mineral, adds the author, I have learned, that Mr. Lowitz has analysed the same subtance. I know not what are precisely the proportions of the principles found by him: but, to judge by the note on the subject inserted in the Journal de Physique, the results he obtained nearly agree with mine; since it says, that he found in the Siberian mineral more than half its weight of oxide of chrome, iron, alumine, and a little silex.

SCIENTIFIC NEWS.

THE Jacksonian Prize of the Royal College of Surgeons in London, for the year 1811, has been adjudged to Mr. Joseph Hodgson, of Bucklersbury, London, for his dissertation on the diseases of Arteries and Veins; comprising the pathology and treatment of Aneurism and wounded Arteries.

To Correspondents.

Mr. Lydiatt's paper is obliged to be deferred till next month, the plates for both numbers of the Journal for the present month having been some time in hand, when it was received.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1812.

ARTICLE I.

A Description of the Smicrologometer for ascertaining the Tenacity of Metals, Silk, Cotton, and Linen Threads, &c. invented by Mr. E. LYDIATT, *Professor of Mechanics, and Lecturer on Metallurgy and Manufactures, &c.*

To W. NICHOLSON, Esq.

SIR,

IN the course of much practice in the application of metals to the purposes of delicate machinery, I have frequently found it necessary to ascertain their comparative tenacity; and also of alloys, in different proportions of combination. Tenacity of metals desirable to be known,

To do this, I have adopted the process employed by Muschenbroeck and other foreign experimenters on this subject; that is, by drawing the metals, to be subjected to experiment, into wires of a given thickness, and then suspending them vertically by one extremity, while weights were attached to the other; which weights were increased by fractional additions, till a separation of the particles took place. and sought by the suspension of weights from wires.

The results obtained, however, by these experiments, I never found satisfactorily correct, on account of the impossibility of increasing the weight attached by sufficiently small The results not satisfactory.

small proportions, as not in most instances to exceed what was absolutely necessary to effect the required purpose.

The spring steelyard defective.

I began therefore to consider, that the employment of a spring might be more effectual for my purpose than weights. I according tried a common spring steelyard, which, however, I found objectionable, from the difficulty of noting the breaking point precisely on the graduated slide, by reason of its being instantantaneously carried back by the recoil of the spring: an objection affecting the accuracy of the experiment as materially as that above stated against the weights.

A screw combined with the spring effectual.

After a little farther consideration on the subject, I contrived a method of combining a screw with the spring, in a way which I have found effectually to give a force capable of being approximated to the required point by the most uniform and gradual increments; constituting thereby an experimental accuracy, which I believe impossible to be obtained from weights however applied.

In this arrangement, by the rotary motion of the screw, an index is moved round a quadrantal scale, graduated with lbs. and their decimal parts; and this index remaining stationary at the breaking point, an accurate indication of the quantity of force is obtained, and can be noted at pleasure.

The principle of extensive application.

This contrivance being considered capable of an extensive and useful application, is the reason I solicit its publicity through the medium of your Scientific Journal.

The whole of the Smicrologometer is represented in the annexed drawing, which I hope will be rendered intelligible by a short explanation.

The apparatus described.

Fig 1, Plate III, is a perspective view.—AA a piece of wood of any required length and thickness, at one extremity of which is screwed the brass plate BB. On this are fixed the two standards CC; and into these is pivotted the endless screw *d*, furnished with the nut *e*, which, on turning the screw by the micrometer head *f*, moves backward and forward at pleasure.

This nut has a slider *g* attached to it under the screw, and passing through a hole in the standard C, in which it slides freely as the nut is moved.—*h*, an immovable round piece

piece of brass passing through a hole in the lower extremity of the nut, and having its ends secured in the standards C C, is intended to steady the motion. G the tube containing the spiral spring represented fig. 2. Down the middle of this spring passes the rod or slide, fig. 3. *a* is a button of brass of the same diameter as the interior of the tube G, and screwed on the end of the slide, so that, when in its proper situation, it may rest on the extremity of the spring. *b*, fig. 3, a round piece of brass, which screws in and closes the end of the tube after the spring is in; it has a perforation in the middle, through which the extremity *c* of the slide freely passes, and is connected to the slider *g* of the nut *e*, fig. 1.

The apparatus described.

By this arrangement it will be seen, that as the tube G, containing the spring, is attached to the movable nut *e*, and freely supported by the end of the brass plate B B, which is turnedup at a right angle for this purpose, it must move altogether backward or forward as the nut is moved; but if the wire *i* have one end coiled round the pin *k*, which is made to turn for this purpose in a piece of brass screwed into the end of the tube, and the other round a similar pin *l*, which is inserted in a brass slider, that moves horizontally, through the standard *o* for the purpose of adjusting its length; and, while in this position, if the screw *f* be turned so as to move the nut back, the slide will be drawn out, and the tube held in its original position by the wire *i*; which will acquire a tension equal to the resisting power of the spring, as compressed between the round piece of brass *b* and the button *a* fig. 3. If the screw continue to be turned slowly, the tension of the wire will consequently increase, till the cohesive attraction of its particles be overcome by the expansive force of the spring.

This expansive force, being the measure of tenacity in the wire, will be indicated by the index *m*, consisting of a small brass quadrant and pointer as at fig. 4, fixed on the quadrantal scale *n n n*, round which it moves as the screw turns, in consequence of teeth on the edge, which work in the threads of the screw. The index moving in this way is not influenced by the recoil of the tube, when the wire breaks, but remains at the degree it has been carried to on the

the scale, till the screw is turned the contrary way to bring it back to the zero. Figs 5, 6, 7, &c. represent such parts as may not perhaps be quite intelligible in the perspective view of the instrument: namely the screw and nut, standards, &c., all of which, I hope, will be understood, without a more diffuse description.

Applicable to threads or cords of any kind:

The application of the smicrologometer to ascertain the tenacity of metals being understood, it will be easy to conceive, that it may be readily employed in the same way, to determine the strength of silk, cotton, or linen threads, &c., affording thereby means of calculating with facility the force any combination of them will sustain in cordage, cloth, &c.

and to stringed instruments of music.

It will likewise be found a desideratum to those manufacturers of stringed musical instruments, who wish to approximate to perfection on scientific principles.

It being now determined by experiment, that the transmission of clear and continuous sounds from piano fortes, harps, &c., depends very much on the due proportion of their component parts, and more particularly those to which the wires or strings are immediately attached; it becomes in consequence necessary, that the exact tension, of strings producing different sounds should be known as correctly as their lengths, in order, that such proportions may be given as will exactly support the aggregate tension without impeding the vibrations by unnecessary quantities, of metal or wood.

Mode of application to these.

To adapt the smicrologometer to this purpose, nothing is necessary, but to affix a monochord scale with a movable bridge on the top of the piece of wood A A: when you have an instrument that will determine at once the length and tension of any string, or wire, to the highest degree of accuracy, that is capable of practical application.

Experiments with the instrument promised.

Satisfied that every invention and discovery, having the prospect of opening a shorter and less intricate avenue to truth, comes with a fair claim to approbation from all who are interested in the advancement of science; I shall not hesitate to lay before the public, in some future paper, the results of a number of experiments made to ascertain, more correctly than has hitherto been done, the relative tenacity of the different metals, and their alloys: accompanied with such

such practical observations on their general properties, as may serve to show the importance of a more particular attention to this subject, than has hitherto been thought necessary in practical mechanics.

E. LYDIATT.

London, April the 15th, 1812.

II.

A Chemical Account of an Aluminous Chalybeate Spring in the Isle of Wight. By ALEXANDER MARCET, *M. D. F. R. S. one of the Physicians to Guy's Hospital, and Member of the Geological Society.*

(Concluded from p. 66.)

SECT. IX. *Sulphate of Alumine.*

1. FIFTY grains of residue † were boiled in two successive lixivia of caustic potash (as in sect VIII, 1), so as to take up all the alumine present; the residue was separated and well washed, and the washings were added to the alkaline solution. The clear liquor had a brownish colour, and on being tried with muriatic acid and prussiate of potash, a blue tinge was produced, which appeared to have arisen from a few particles of oxide of iron, which were suspended in the lixivium rather than actually dissolved; for the solution being left at rest for some time, these particles subsided. Residue treated with caustic potash,

2. To the clear alkaline solution muriate of ammonia was added, till no farther precipitate took place; the precipitate was edulcorated and collected in a filter. It was and precipitated by muriate of ammonia.

† These fifty grains had been previously boiled in neutral carbonate of ammonia, in order to separate the magnesia, as will be detailed hereafter. The previous intervention of a carbonated alkali renders the subsequent application of caustic potash for the separation of the alumine more unexceptionable, as a solution of caustic potash might redissolve a small portion of the lime, if it were not previously carbonated.

white

white and gelatinous. Caustic potash being added to the clear fluid, ammonia was disengaged, showing that it contained an excess of muriate of ammonia; and acetic acid being added to another portion of the same liquor, no turbidness appeared, both circumstances showing, that all the alumine was precipitated. This precipitate being dissolved in muriatic acid, in order to separate a minute portion of silica, which it contained*, and being again precipitated by succinate of ammonia with excess of ammonia, formed a gelatinous mass, which being edulcorated, dried, and ultimately heated to redness, weighed 2·4 grains.

Another portion.

3. Another portion of residue, weighing thirty grains, being treated in a manner exactly similar to that just described; with this exception, that the redissolution of the alumine in muriatic acid and its subsequent precipitation by succinate of ammonia, were omitted; the gelatinous precipitate, heated to redness, weighed 1·4 grain †, which afforded as close a coincidence with the former result as may be well expected in processes of this kind.

Crystals of alum obtained by adding potash.

4. Having never been able to obtain, by the mere evaporation of the water, any appearance of crystals resembling alum, I was desirous for the sake of obtaining farther evidence on the subject, to bring the sulphate of alumine to a crystallized state, by artificially supplying what I conceived to be wanting for the completion of that process. For this purpose, having dissolved about thirty grains of residue in distilled water, I added to the filtered solution two or three drops of a solution of carbonate of potash, and evaporated it very slowly; crystals were thus obtained, dispersed in the saline mass, which, though of a size scarcely exceeding that of a pin's head, had a distinct octohedral

* The particulars of the manner in which the silica is separated, by the intervention of muriatic acid, will be detailed under the head Silica, in another part of this paper.

† The real weight was 1·6 grain, but 0·2 of a grain were deducted, on account of the quantity of silica known, by other experiments, to have been present, as will be seen under the head Silica. It may be proper to mention, that the gelatinous precipitate, during its gradual desiccation, shrunk into small fragments resembling coarsely pulverized glue, an appearance which is well known to characterize alumine.

form

form, and, when separated and chemically examined, had all the properties of alum.

5. With regard to the proportion of sulphate of alumine, contained in the water, it will be seen, that by connecting together the results of the experiments just related (1, 2, 3), eighty grains of residue, or a pint of the water, yield 3·8 grains of alumine heated to redness, which, according to the proportion of twelve parts of iginted alumine in one hundred parts of crystallized alum*, would be equivalent to 31.6 grains of the alum in each pint of the water‡. Proportion of sulphate of alumine.

SECT. X. *Sulphate of Lime.*

1. Some of the former experiments (sect. III, d and g) had shown, beyond all doubt, the presence of selenite; and indeed, from the general composition of the water, lime could scarcely be supposed to exist in it in any other form of combination. Examination for sulphate of lime.

To ascertain the quantity of this substance, a variety of methods was used, the principal results of which I shall cursorily relate.

2. It would have been in vain, in this instance, to have applied, without any previous step, oxalate of ammonia, the usual test of lime, in order to obtain an accurate estimate of the quantity of lime present in the water; for as oxalic acid also acts upon iron, some ambiguity would necessarily have occurred. Indeed that oxalate of ammonia did not, in this case, react upon the lime in the manner that it usually does, had been noticed, (sect. III, f, g) in some of the preliminary experiments†.

3, It

* These are the proportions stated by Mr. Kirwan, and which I obtained myself on a former occasion (See the Analysis of the Brighton Chalybeate)

‡ It is scarcely necessary again to observe, that the sulphate of alumine contained in the water does not appear to exist there in the state of alum; but it is perhaps better to express the quantity of alumine by the quantity of alum which it would form, as the crystallized state of the salt affords a much more precise standard of comparison.

† By adding a considerable quantity of oxalate of ammonia, and concentrating the solutions by heat, the whole of the lime appeared to be precipitated, together with a portion of iron; but in order to obtain Iron precipitated with lime by oxalate of ammonia.

SECT. XI. *Inferences obtained from the application of Alcohol.*

Examination for muriates by alcohol.

1. Having ascertained (sect. III, k), that a small quantity of muriatic acid was present in the water, it became desirable, before proceeding any farther, to discover, by the agency of alcohol, which has the well known property of dissolving the earthy muriates, with what bases this acid was combined. With this view, 20 grains of residue were digested in successive quantities of alcohol of great purity, and the solution filtered. The residue, by this operation, acquired a lighter colour and a more pulverulent appearance. Part of this residue being treated with muriatic acid and oxalate of ammonia, oxalate of lime was precipitated; and another portion being treated with neutral carbonate of ammonia and phosphate of soda, some magnesia was precipitated in the form of triple phosphate, circumstances which confirmed the presence of lime in the form of selenite, and that of magnesia, in the form of sulphate or Epsom salt.

2. The alcoholic solution being evaporated to dryness, a yellowish deliquescent residue was obtained, which, being dried at 160°, weighed 0·9 of a grain. Water being added to this residue, a small portion of it remained undissolved. The filtered watery solution was yellowish, though perfectly transparent, and, being examined by the usual reagents, appeared to contain iron, sulphuric acid, and muriatic acid, with imponderable vestiges of lime and magnesia, without any trace of alumine.

Results.

3. From these circumstances it was inferred, that the only deliquescent salts yielded by the residue, in ascertainable quantities, were sulphate of iron, and muriate of iron, both of which had probably been formed in consequence of some new orders of attraction taking place during the process of evaporation to which the water had been subjected*.

SECT.

(which he finds to be equal to 128 grs. dried at 160°), give 102·5 grs. of oxalate of lime dried at 160°, corresponded to 124 grs. of sulphate of lime dried at the same temperature. [See Journ. vol. XXVI. p. 278.]

* Namely, the red sulphate from the hyperoxigenation of the iron, and the muriate from the decomposition of muriate of soda, as will be explained hereafter.

SECT. XII. *Sulphate of Magnesia.*

Examination for magnesia.

1. The presence of magnesia * was acertained beyond all doubt, in the following manner:

50 grains of residue minutely pulverized were boiled in a solution of neutral carbonate of ammonia, so as to decompose all the sulphate of iron and earthy salts, and dissolve all the magnesia which might be present†. This process was, of course, attended with considerable effervescence, and when this had subsided, the liquor was filtered. The clear solution deposited on standing a brownish sediment, which was separated, and proved to be oxide of iron. The residue left in the filter had passed from a greenish-yellow to a pale brown colour.

Indication of ammoniaco-magnesian phosphate.

2. Phosphate of ammonia being added to the clear solution, a precipitate appeared, having all the characters of the ammoniaco-magnesian phosphate; and in particular, that of forming white stripes on the inside of the vessel when scratched with a pointed instrument. This precipitate, dried at a temperature of about 120°, weighed 1·9 grain‖, and being made red hot in a platina crucible, was reduced to exactly 1 gr. = 0·385 of a grain of pure magnesia = 2·26 grains of crystallized sulphate of magnesia in 50 grains of residue, or 3·63 grains in a pint of water‡. The magnesian

Proportion of sulphate of magnesia.

* The presence of this earth in the form of sulphate had already been proved by the application of alcohol, (sect. XI, 1).

† It is scarcely necessary again to state here the well known fact, that carbonate of ammonia, when fully saturated with carbonic acid, has the power of dissolving magnesia.

‖ In a subsequent experiment, in which the water itself, instead of the residue, was treated in the same manner with neutral carbonate of ammonia, the quantity of magnesia appeared somewhat greater; but the difference did not amount to more than one tenth of a grain.

Proportion in which magnesia and phosphoric acid combine.

‡ It will be necessary here to state the grounds of this computation, which will afford me an opportunity of relating some general results concerning the proportions in which magnesia and phosphoric acid combine.

By dissolving 11·82 grains of the purest magnesia (perfectly free from carbonic acid and water) in muriatic acid, and precipitating it by a mixture of phosphate of ammonia, and neutral carbonate of ammonia, I obtained 65 8 grains of the triple phosphate dried by exposure for near forty-eight hours to a temperature which never exceeded

magnesian phosphate became slightly brownish during the calcination, owing to the presence of a few particles of iron, the quantity of which was too minute to be ascertained.

Sect. XIII. *Precipitation of the sulphuric and muriatic Acids, with a view to ascertain their quantity.*

Before drawing any ultimate conclusion respecting the contents of the water and the proportions of its ingredients, I found it necessary to ascertain the quantities of sulphuric and muriatic acids which it contained, in order to enable me to try how far these quantities might coincide with the conclusions obtained by the separation of the basis, and also to assist me, as will be seen hereafter, in forming certain inferences with regard to the alkaline salts. For this purpose I made the following experiments.

Examination of the quantity of the acids.

1. To

ceeded 120°, a degree of heat under which this salt appears to retain the whole of its ammonia. These 65·8 grains of triple salt, being exposed for half an hour to a strong red heat in a platina crucible, were reduced to 30·8 grains. The salt appeared then in the form of a friable cake or loose aggregate, a fragment of which, on being urged by the blowpipe, ran into a white opaque vitreous globule, without any farther diminution of weight. In its friable state it was readily dissolved by muriatic acid; in its vitrified form it required heat and trituration. This salt was perfectly tasteless, and showed no attraction for water. With regard to the proportions of acid and base to be inferred from this experiment, it is obvious, that, if 30·8 grains of phosphate of magnesia contain 11·82 grains of earth, the remainder, viz. 18·98 grains, represents the proportion of phosphoric acid; which is equivalent to 38·37 grains of magnesia in 100 of phosphate. In another experiment conducted in a similar manner, the magnesia amounted to 38·7 grains, so that, by taking the mean between these two very nearly similar results, we have the following proportions, viz.

Magnesia . 38·5 }
Phosphoric acid 61.5 } in 100 grains of ignited phosphate of magnesia.

We may infer therefore, that one grain of phosphate of magnesia, the quantity yielded by twenty grains of residue, indicated 0·385 of pure magnesia; and if, according to the statements of Kirwan and Wenzel (which very nearly agree) one hundred grains of crystallized sulphate of magnesia contain seventeen grains of magnesia, 2·26 grains of that salt will be the quantity corresponding to 0·385 of a grain of magnesia. And I have the satisfaction of observing, that the proportions

Sulphuric acid. 1. To four ounces of the water was added nitrate of barytes till the whole of the sulphuric acid was precipitated; the sulphate of barytes thus obtained being carefully edulcorated, and heated to redness in a platina crucible, weighed 18·5 grains, which correspond to 74 grains of sulphate of barytes from a pint of the water.

Muriatic acid. 2. Four ounces of the water were treated with nitrate of silver as long as any precipitate appeared, and the muriate of silver thus obtained, being well edulcorated, and afterwards brought to a state of incipient fusion by the heat of an Argand lamp, weighed 2·05, which is equivalent to 8·2 grains of luna cornea, or four grains of muriate of soda*, in each pint of the water†.

SECT. XIV. *Sulphate and Muriate of Soda.*

Examination for alkaline salts. 1. The mode in which I first attempted to ascertain the presence of alkaline salts in the water was that alluded to in a former part of this paper, which consisted in precipitating the iron and the earths by subcarbonate of ammonia, evaporating the clear solution to dryness, heating the dry mass to redness, with a view to drive off the sulphates and muriates of ammonia, redissolving the residue in water, and evaporating again very slowly in order to obtain crystals. But the saline mass yielded by this process did not crystallize regularly, and, on being examined by reagents, was found to contain only sulphate of soda, with minute quantities of sulphates of alumine and magnesia, which had escaped the action of the carbonate of ammonia.

portion, obtained by Dr. Henry, of one hundred grains of ammoniaco-magnesian phosphate dried at 90°, for one hundred and eleven grains of crystallized sulphate of magnesia, would have led to a very similar result. (See Dr. Henry's 'Analysis of several varieties of salt,' in Philos. Trans. for 1810, page 113.) [Journ. vol. xxvi, p. 277.]

* I have found by direct experiments, that one hundred grains of pure muriate of soda heated to redness, and decomposed by nitrate of silver, yield 241·6 grains of luna cornea heated to fusion.

† The same experiment was tried three times upon different specimens of the water, and I here give the average. The smallest quantity of luna cornea obtained was two grains, and the largest 2·5 grains, a difference too great to arise from mere inaccuracy. From this and several other circumstances I have reason to suspect, that the water is subject to occasional variations in the proportions, as well as in the aggregate quantity of its solid contents.

2. In hopes of obtaining more satisfactory results, I had recourse to the following process: five ounces of the water were boiled with a solution of succinate of ammonia till the whole of the iron and alumine were precipitated*. The lime was precipitated by oxalate of ammonia, and the magnesia by ammonia. The solution was then concentrated over a lamp, and gradually evaporated to dryness in a platina crucible. A white pungent smell arose, and on raising the heat to redness, these fumes took fire and burnt with a blue flame, till the whole was fused and reduced to a fixed saline mass mixed with a black coaly matter. Distilled water was poured upon this mass, and the solution filtered. This clear solution being now evaporated and dried at a gentle heat, so as to obtain the salts in a crystallized state, the mass weighed 6·3 grains†, which would give 20 grains of alkaline salts in a pint of the water. The centre of this mass exhibited no distinct crystallization, though from its appearance and disposition to effloresce, it evidently contained sulphate of soda; but the circumference was strewed with numerous and perfectly regular crystals of muriate of soda‡. Treatment with succinate of ammonia.

3. This

* This is a long operation, because the iron does not combine with the succinic acid at a low degree of oxigenation, so that the mixture must be long digested with access of air, or repeatedly boiled and allowed to stand in the air for some hours during the intervals, before the process can be completely effected. This operation necessarily requires one or two days, but is remarkably accurate as to the precipitation of both the iron and alumine.

† This was the combined result of two separate experiments tried on three and two ounces of the water, the first of which yielded 3·5 grains, and the other 2·8 grains of alkaline salts.

‡ This result shows the compatibility of muriate of soda with sulphate of iron, the latter being in excess, which has been questioned by some chemists. Being desirous of obtaining a confirmation of this by a direct experiment, I mixed together solutions of two parts of sulphate of iron and one part of muriate of soda. The mixture became yellowish, and on applying heat reddish flakes subsided. On separating these by filtration, and repeating this process two or three times, I nevertheless obtained by evaporation distinct crystals of muriate of soda, partly cubic, partly octohedral, deposited in the centre of a saline yellowish mass, without any appearance of efflorescence or of any thing resembling sulphate of soda. Therefore muriate of soda is compatible with sulphate of iron, although these two salts Muriate of soda compatible with sulphate of iron.

Properties of the saline mass.

3. This saline mass being dissolved in water, the solution had the following properties:

a. It was neither acid nor alkaline.

b. Its most obvious taste was that of muriate of soda.

c. It formed copious precipitates with nitrate of barytes, nitrate of silver, and nitrate of lime.

d. Oximuriate of platina, oxalate of ammonia, and prussiate of potash, produced no precipitate whatever.

Therefore the only salts contained in this solution were sulphate of soda, and muriate of soda.

Perhaps muriate of soda only present.

4. As to the proportions of these two salts, it would have been easy to ascertain them by precipitating their acids. But it occurred to me, that the sulphate of ammonia formed in the solution by the ammoniacal salts, which had been introduced for the precipitation of the earths, had probably reacted upon the muriate of soda when urged by heat, so as to decompose it partially, and form the sulphate of soda obtained by the process just described; so that muriate of soda might perhaps in fact be the only alkaline salt contained in the water.

Proof of the action of sulphate of ammonia on muriate of soda.

5. In order to ascertain this, another portion of the chalybeate having been treated in the way just described with succinate of ammonia, the residue was gradually desiccated, and then heated to redness in a platina crucible, which was at first kept closed, in order to retard the escape of the sulphate of ammonia, and thus promote its action on the muriate of soda. The remaining mass, being dissolved and very slowly crystallized, assumed the form of clusters of regular prismatic efflorescent crystals of sulphate of soda, among which scarcely any vestige of muriate of soda could be discovered.

Proportions of the sulphate and muriate determined indirectly.

6. The decomposition of muriate of soda by the above process being thus well established, it became necessary to determine the proportions of sulphate and muriate of soda by some

salts evidently exert some degree of action on each other, as appeared from the change of colour and the formation of reddish flakes, which I suppose to be subsulphate of iron. I may take this opportunity of mentioning, that by an analogous experiment on sulphate of iron and muriate of alumine, and by the assistance of alcohol, I satisfied myself that these two salts *could not* exist together.

some less direct method; and the expedient which appeared the most appropriate was that of inferring the point in question from a reference to the quantities of acids as estimated in the preceding section. Thus as it was obvious that, whatever the case might be with regard to sulphate of soda, the presence of muriate of soda in the water was unquestionable; and as the whole quantity of muriatic acid discovered in the water (§XIII, 2), corresponded to a quantity of muriate of soda which fell far short of the sum total of alkaline salts, I naturally inferred, that the whole of the muriatic acid was united with soda, and that the water must also contain a quantity of sulphate of soda sufficient to complete the 20 grains of alkaline salts, which the experiments just related had shown to exist in each pint of the water.

7. Since therefore the whole of the muriate of soda, as was before computed (§XIII, 2), amounted only to 4 grains in a pint, the quantity of crystallized sulphate of soda contained in each pint of the water will be 16 grains.

Sect. XV. *Comparison of the quantities of Acid actually obtained from the water by precipitation, with the quantities inferred from the precipitation of the basis.*

1. It appears evident, from all that precedes, that the only acids contained in the water are the sulphuric and muriatic. The whole of the muriatic acid having been shown to exist in the form of muriate of soda, nothing farther remains to be said on this head. But it will be curious to examine how far the total amount of sulphuric acid, obtained from a portion of the water, would coincide with that which might be inferred from the quantities of bases, with which it was combined. This inquiry will give rise to the statement of certain results respecting the proportions of acid and base in some of the salts concerned, and the precipitates obtained from their decomposition, which, from their general import in chemical analysis, appear to deserve some attention.

Quantities of acid obtained by precipitation compared with that inferred from the bases.

2. It was ascertained by a direct experiment (§ XIII, 1) that the whole of the sulphuric acid, contained in a pint of the water, formed, when precipitated by a barytic salt, a quantity of sulphate of barytes, which, after being ignited, weighed 74 grains.

I shall

I shall now recapitulate the several sulphates discovered in the water, and from the quantities of each compute the quantities of barytic sulphate, which would result from their decomposition.

Sulphates in the water.

Sulphates contained in a pint of the Water.

	Sulph. of baryt. ignited.
Sulphate of iron (§VIII, 6) 41·4 grs. crystallized =	31·8 grs.*
Sulph. of alumine (§IX, 5.) 3·8 grs. ign. alumine =	17 7 do. †
Sulph. of lime (§ X, 3) 10·17 grs. dried at 160° =	13·9 do. ‡
Sulph. of magnesia (§ XII, 2) 3·63 grs. crystal. =	4·0 do. ‖
Sulph. of soda (§ XIV, 7) 16·9 grs. crystallized =	11·6 do. §
Total amount of the sulphate of barytes	79·0 grs.

* These proportions were deduced from the following experiment: 50 grains of crystallized green sulphate of iron were dissolved in water, and nitrate of barytes was added as long as any precipitate took place. The sulphate of barytes, after being carefully edulcorated and heated to redness in a platina crucible, weighed 38.5 grs. Therefore 50 : 38·5 : : 41·4 : 31·8.

Proportions of acid and base in pure sulphate of alumine.

† It may be recollected that 3·8 grs. of ignited alumine would, according to the proportion before stated (Sect IX, 5,) correspond to 31·6 of crystallized alum. I found by a direct experiment, that 100 grs. of regular octohedral crystals of alum formed by gradual deposition from a staturated solution of common alum, being dissolved in water and precipitated by muriate of barytes, produced 88·2 grs. of ignited sulphate of barytes; so that the 31·6 grs of alum would correspond to 27·8 grs. of the barytic sulphate. This, however, could not be an accurate estimate of the real quantity of sulphuric acid, since the sulphate of alumine does not exist in the water in the state of alum.

With a view to learn the proportions of acid and base in *pure* sulphate of alumine, I made the following attempt. A quantity of alumine (which had been prepared by precipitation from alum, redissolution in muriatic acid, and second precipitation by carbonate of ammonia, and appeared to contain no impurity except a vestige of muriatic acid), was dissolved in sulphuric acid, and the solution evaporated to siccity. When reduced to the consistence of a thick sirup, and allowed to cool, the saline mass congealed into a hard whitish deliquescent cake, capable of being pulverized. This was redissolved and reevaporated four successive times, and the last time was made redhot, in order to expel the excess of sulphuric acid, which always appeared to prevail. By this last operation a portion of the salt was decomposed and rendered insoluble in water, in spite of which the remainder still exhibited signs of acidity. The clear solution of this mass being divided into

two

3. It appears therefore, that the aggregate of the analytical results would indicate 79 grs. of ignited sulphate of barytes, instead of the 74 grs. obtained by a single direct operation. This difference I apprehend to be in a great degree owing to my estimate of the proportion of acid in sulphate of alumine being overrated, from the circumstance of not having been able to obtain a neutral sulphate of alumine in the experiment just related from which that estimate was deduced.

Difference between the sulphate of barytes calculated and obtained.

Sect. XVI. *Silica.*

1. During the various solutions of the residue in acid, I had repeatedly observed, that, beside the selenite, (the solution of which was attended with some difficulty, and re-

Examination for silex.

two equal portions, one of which was precipitated by succinate of ammonia, and the other by nitrate of barytes, yielded 4·5 grs. of ignited alumine, for 21 grs. of ignited sulphate of barytes. From which it may be inferred, that the 3·8 grs. of ignited alumine, found in a pint of the water, were combined with a quantity of acid equal to 17·7 grs. of ignited sulphate of barytes. But it is assumed in this computation, that the artificial sulphate of alumine subjected to analysis, was in the same state of combination as that which exists in the water, a supposition which may not be strictly accurate.

‡ The quantity of sulphate of barytes, produced by the precipitation of a given quantity of sulphate of lime, was ascertained in the following manner: some pulverized crystals of native selenite, apparently perfectly pure, were dissolved in water and afterwards slowly precipitated by evaporation. The object of this previous operation was to obtain the sulphate of lime in a state more fit for subsequent redissolution. Fifteen grains of this selenitic residue, dried at a red heat, were dissolved in water, slightly acidulated by muriatic acid, in order to supersede the necessity of using a large quantity of water; and the solution, after being neutralized by pure ammonia, was precipitated by muriate of barytes. The sulphate of barytes, thus obtained, weighed, after careful edulcoration and ignition in a platina crucible, 26·75 grs. which are equivalent to 175·6 grs. of sulphate of barytes for 100 grs. of ignited sulphate of lime.

Proportion of sulphate of barytes to sulphate of lime.

‖ According to Dr. Henry 100 grs. of crystallized sulphate of magnesia give 111 grs. of ignited sulphate of barytes. See Philos. Trans. 1810, p. 114. [Journ. vol. xxvi, p. 278.]

§ These proportions were deduced from the following experiment: 40 grs. of crystallized sulphate of soda, being dissolved in water and precipitated by nitrate of barytes, the sulphate of barytes, well edulcorated and ignited, weighed 29·1 grs.

 quired

quired a considerable quantity of water) there always remained a small proportion of earthy matter, which resisted all solvents, caustic potash excepted. This insoluble matter, I had thought from some of the first trials, amounted to about 1 gr. in 100 of the residue; but from some subsequent experiments in which the silica was separated by caustic potash, there appeared to be reason to suppose, that this estimate was rather overrated. I shall relate the process, to which, after various trials, I gave the preference.

Silex dissolved by caustic potash, and precipitated by muriate of ammonia.

2. 50 grains of residue being boiled with very dilute muriatic acid, a white flocculent substance remained undissolved, upon which neither acid nor water could make any impression. This substance, being separated and boiled in a solution of caustic potash, readily redissolved with the exception of a few particles of highly oxidated iron, which subsided. Muriate of ammonia * being added to the clear alkaline solution in sufficient quantity to saturate the whole of the potash with muriatic acid, the white flocculent substance reappeared, which after being well washed, and heated to redness, weighed between 0·3 and 0·4 of a gr. This substance when heated with alkali ran into a vitreous globule, and muriatic acid being poured upon this, the alkali was dissolved, and the earthy matter remained untouched. It was therefore silica, the quantity of which may be estimated at 0·7 of a gr. in a pint of water†.

On

* This precipitant, which was, I believe, first proposed by Mr. Chenevix, is much more appropriate than acids, because if an excess of acid be incautiously added, the precipitate is redissolved: while with muriate of ammonia an excess of the test is attended with no inconvenience.

Another examination for silex.

† The presence of silica was also shown, and its quantity attempted to be ascertained by the following process. A portion of residue was boiled in caustic potash: this dissolved not only the silica, but also the alumine; both these earths were precipitated from the alkaline solutions by muriate of ammonia, and separated; muriatic acid being now added, both the silica and alumine were redissolved (for silica, just precipitated from its solution, and not desiccated, is soluble in acid); and this solution being evaporated to dryness on a water-bath, by which means the silica parts with its acid and becomes insoluble, the muriate of alumine was washed off by distilled water, and the silica remained undissolved. This method, though affording a very useful

SECT. XVII. *Conclusion.*

viewing and connecting together all the foregoing t appears that each pint, or sixteen-ounce measure uminous chalybeate, contains the following in- :

Ingredients in a pint of the water.

bonic acid gas three tenths of a cubic inch.	GRAINS
of iron, in the state of crystallized green e	41·4
of alumine, a quantity which, if brought to e of crystallized alum, would amount to	31·6
of lime, dried at 160°	10·1
of magnesia, or Epsom salt, crystallized	3·6
of soda, or Glauber's salt, crystallized	16·0
f soda, or common salt, crystallized	4·0
	0·7
	107·4

ot acquainted with any chalybeate or aluminous the chemical history of mineral waters, which ompared, in regard to strength, with that just The Hartfell water, and that of the Horley- w near Halifax, both of which appear to be ana- this in their chemical composition, and were con- the strongest impregnations of the kind, are stated arnett to contain, the one only about 14 grs. and 10 grs. of saline matter in each pint.

It is analogous to the Hartfell and Horley green, but the strongest of its kind known.

bt therefore can be entertained, that the water, e subject of this essay, will be found to possess in inent degree the medical properties, which are elong to the saline substances it contains. Indeed ars to be in this spring rather a redundance than y of power; and it is probable, that in many in- will be found expedient to drink the water in a diluted

s of discrimination, must obviously be liable to inaccuracy tions, when very minute portions of silica are to be sepa- onsiderable quantities of almine. This however was the hich I trusted on a previous occasion (IX, 9.) to free the the silica which was mixed with it.

diluted state; while in others, when it may be desirable to take in a small compass large doses of these saline substances, it will be preferred in its native undiminished strength.

III.

Account of some new Eperiments on Wood and Charcoal: by Benj: Count of Rumford, *F. R. SS. L. and E. M. R. I. A. &c.**

Various kinds of wood procured for drying.

HAVING had occasion to dry several kinds of wood, to ascertain how much water was contained in them, I procured a piece of each kind six inches long and half an inch thick, and planed off some pretty thin shavings, which I kept to dry for eight days in a room, the temperature of which was constantly about 60° F. The wood had been previously drying two or three years in a joiner's workshop.

Exposed to a moderate heat in a stove.

Of each kind of shavings I took 10 gr. [154·5 grs.] which I placed on a china plate in a kind of stove made of sheet iron; and heated them moderately by a small fire under the stove for twelve hours, after which they were suffered to cool gradually during twelve hours more. The stove, being surrounded with brick-work, was still hot twelve hours after the fire had been extinguished.

Loss of weight.

On taking out the china plates in succession, and weighing the shavings anew, their weight was found to be diminished about one tenth, some a little more, others a little less. When the shavings were put into the stove, their weight was 10 gr., when taken out it was about 9. Their colour was not perceptibly altered, and they had no appearance of having been exposed to a strong heat.

Heated again.

Desirous of knowing how far the drying of wood might be carried, I replaced them all in the stove, which I heated as before, neither more nor less, for twelve hours, and afterward left to cool slowly for twelve hours.

On

* Read at the meeting of the first class of the French Institute, Dec. the 30th, 1811. This, as well as the following, is translated from the original, transmitted by the Count, and not yet published in France.

Change of colour.

On taking out the shavings the next day, they had all hanged colour more or less: from a yellowish white they ad become light brown, dark brown, more or less yellow, nd some of a fine purple.

Weight after the second heating.

Their weight, which was at first 10 grs. was now found be

Oak	7·16	Cherry	8·60
Elm	8·18	Linden	7·86
Beech	8·59	——— (after having been in the open air twenty-four hours)	8·06
Maple	8·41	Male fir	8·46
Ash	8·40	Female fir	8,66
Birch	7·40		
Service	8·46		

Attempt to char a portion by a moderate heat.

Wishing to know whether the wood might not be re- uced to charcoal by continuing the moderate heat of the ove a long time, I took half the linden shavings, which eighed 4·03 gr.; placed them in a china saucer, sup- orted by a cylindrical earthen vessel three inches in ameter, and four inches high; put this on an earthen ate, and covered it by a glass jar, six inches in diameter, d eight inches high. On the earthen plate was a layer of hes, about an inch deep, serving to close the mouth of e jar slightly.

This little apparatus being placed in the stove, it was eated a third time for twelve hours; and then left twelve ours without fire, to cool gradually.

Results.

On taking out the apparatus I found, that the wood was ecome perfectly black; and that the glass jar was yellow- h, and its transparency diminished.

Loss of weight.

On weighing the shavings, which retained their original gure completely, I was surprised to find, that they eighed only 2·21 grs. As they were the remains of 5 gr. wood; and as, from the experiments of Messrs Gay- ussac and Thenard, I had expected to find in this wood at ast fifty per cent of charcoal; I did not think it possible, reduce the weight of the shavings to less than 2·5 gr., rticularly with the moderate heat I employed.

To

Heated a fourth time.

To clear up my doubts, I replaced the apparatus stove, and heated it again as before for twelve hour afterward left it in the stove twelve hours to cool.

Results.

On taking out the apparatus I found, that the sh weighed only 1·5 gr. The jar was less transparen of a blackish yellow colour throughout; but particu its upper part, above the level of the brim of the in which the shavings were. These shavings were st perfect black.

Heated a fifth time.

Having heated the apparatus again for twelve and then left it to cool, I was surprised on taking of the stove the next day to find, that the jar had become clear and transparent. Not the least trace yellow coating, with which its inner surface had b vered, now remained.

State of the wood.

On examining the wood I found, that this al changed its colour. It had assumed a blueish hue, deep, but very different from the decided black before. Its weight was 1·02 gr.

Heated twice more.

I put it twice more into the stove, and each t weight was diminished, so that the 5 gr. of woo reduced at last to 0·27 of a gr., or about a twentieth original weight.

Charcoal may be dissipated by a heat below combustion.

I am persuaded, that I should have diminished more, if I had continued the experiment longer: has been tried long enough to establish this rem fact, *that charcoal can be dissipated by a heat much l has been considered necessary to burn it.*

Experiment with common charcoal.

It may be supposed, that I was very desirous of k whether the same thing would occur to charcoal formed by the usual process. Accordingly I piece of charcoal from my kitchen, heated it to a red heat, and, while it was still red, put it into a mortar, and powdered it. Having passed it through I took 4·03 gr. of the powder, placed it in the heated it in the stove twelve hours, and then left it hours to cool. On taking it out it weighed but 3·8

A second experiment.

As this powdered charcoal was nothing but a co of small bits of charcoal, which were in contact with only by a very small surface compared with that

hs

shavings, I made another experiment, the result of which was more striking and more satisfactory.

Having enclosed in a cloth a quantity of powdered charcoal, that had been passed through a sieve, I beat it strongly in a place where the air was still; and when the air appeared to be well loaded with the fine dust of the charcoal, I placed on the ground a white china saucer, quitted the place, and left the dust to settle.

Charcoal in very fine powder employed.

The saucer was covered with it, so as to appear of a very dark gray.

Before all the dust had settled, I wrote some letters on the saucer with the point of my finger, and these letters were afterward covered with a still finer dust.

I imagined it possible, that the part covered by a very fine dust might be found whitened, while that covered with a stratum of coarser charcoal powder would be found perhaps still black.

The result of the experiment showed, that this precaution was not necessary. All the charcoal powder disappeared completely in the stove, and the saucer came out perfectly white.

The whole disappeared in a low heat,

Another saucer, which had been blackened a little by rubbing it with lampblack, and placed in the stove by the side of that blackened with charcoal dust, came out of the stove as black as it went in. As soon as I saw, that the linden shavings converted into charcoal might be dissipated by the moderate heat of the stove, I suspected, that they had been consumed slowly by a silent and invisible combustion; and that the product of this combustion could be nothing but carbonic acid gas.

at which lampblack did not.

To clear up this point I made the following experiment.

Having procured a stock of very dry birch shavings, in ribands about a twentieth of a line thick, near half an inch broad, and six inches long, I dried them for eight days in a room heated by a stove, where the temperature was about 60° F.; the shavings being laid on a table, at a distance from the stove. Of these shavings thus dried, I took 10 gr., which I placed on a china plate, and heated in the stove, in the manner already described, for 24 hours. When taken out of the stove, they weighed but 7·7gr., and had acquired a deep brown colour inclining to purple. They were still wood however,

Experiment to ascertain whether it were converted into carbonic acid gas.

however, for, though deeply browned, they burned with a very fine flame.

Of these brown shavings I made three parcels, each weighing 2·3 gr. The first was placed in the stove on a white china plate, supported by a tile, but not covered. The second was put into it in a similar manner, except that it was covered with a glass jar, six inches in diameter, and six inches high.

The third parcel was put into a glass vessel, six inches high, but only an inch and a quarter in diameter. This narrow vessel was put into a glass three inches in diameter, and seven inches high; which, being slightly closed with its glass cover, was also placed in the stove on a tile.

As the door of the stove (which is double, the better to confine the heat) does not shut so close as to prevent the free passage of air; and as the china plates, on which two of the parcels were placed, were flat; every circumstance was favourable for the free transmission of the carbonic acid gas arising from the decomposition of these two parcels by slow combustion, and there was nothing to prevent the progress of this operation. But the third parcel being enclosed in a narrow vessel, as this gas is much heavier than atmospheric air, the first portion of this gas arising from a commencement of combustion of the wood could not fail to descend in the vessel toward its bottom, gradually expel the air, and at length fill the vessel completely: and as this sort of inundation by carbonic acid gas could not fail to stop the combustion, I expected to find that this parcel of shavings would be preserved, at least in part, even though both the others should be entirely consumed.

Results. The stove having been heated in the usual manner, I found the next day, that the results of the experiment had been such as I anticipated. The two parcels of shavings placed on the china plates had disappeared entirely; nothing at all remaining, except a very small quantity of ashes, of a white colour inclining a little to yellow.

The yellow ashes in the plate that was not covered with a glass jar were deranged and dispersed by the wind, occasioned by opening the door of the stove too suddenly: but those in the other plate, being protected by the glass, were found all

together,

together. As they still retained their original figure of shavings, though reduced to a very small bulk, this appeared to me a demonstrative proof, that the shavings, whence they arose, had not been burned by a common fire. For this reason, and also on account of their extraordinary colour, approaching very near that of the wood in its natural state, I preserved them, to show them to the class. They weighed only 0·04 of a gr.; and as the shavings, of which they were the remains, weighed 2·987 gr. on coming out of the hands of the joiner, these ashes make only one and one third per cent of the weight of the wood.

The third parcel of shavings, which had been placed in a narrow glass vessel, had not disappeared, but the wood was converted into perfect charcoal. I have the honour to present it to the class, in the same vessel in which it was charred.

As the three parcels of shavings were of the same wood, and equal in weight; as they were exposed together to the same degree of heat, and for the same time; and as the two portions, that were placed so as to facilitate the escape of the carbonic acid gas arising from their decomposition, disappeared entirely; while the third, which was so circumstanced that the escape of this gas was impossible, did not disappear; it seems to me, that there can be no doubt of the cause of the phenomena that presented themselves: and it is certainly a curious fact, that charcoal, which has hitherto been considered as one of the most fixed substances known, can unite itself to oxigen, and form with it carbonic acid gas, at a temperature much below that, at which it burns visibly.

Reasonings on them.

Charcoal less fixed than usually supposed.

IV.

Inquiries concerning the Heat developed in Combustion, with a Description of a new Calorimeter; by the Same.*

ATTEMPTS have been long ago made to measure the heat, that is developed in the combustion of inflammable substances;

Results of experiments on heat from combustion disputable.

* Read at the meeting of the fifth class of the French Institute, Feb. the 24th, 1812.

substances; but the results of the experiments have been so contradictory, and the methods employed so little calculated to inspire confidence, that the undertaking is justly considered as very little advanced.

Unsuccessful attempts of the author.

I had attempted it at three different times within these twenty years, but without success. After having made a great number of experiments with the most scrupulous care, with apparatus on which I had long reflected, and afterward caused to be executed by skilful workmen, I had found nothing however that appeared to me sufficiently decisive to deserve to be made public. A large apparatus in copper more than twelve feet long, which I had made at Munich fifteen years ago; and another scarcely less expensive made at Paris four years ago, which I have still in my laboratory; attest the desire I have long entertained of finding the means of elucidating a question, that has always appeared to me of great importance, both with regard to the sciences, and to the arts.

Simple and accurate method discovered.

At length, however, I have the satisfaction of announcing to the class, that, after all my fruitless attempts, I have discovered a very simple method of measuring the heat manifested in combustion, and this even with such precision, as leaves nothing to be desired.

That the class may be the better able to judge of my method of operating, and the reliance that may be placed on the results of my experiments, I place my apparatus before it.

The apparatus described.

The principal part of this apparatus is a kind of prismatic receiver, eight inches long, four inches and a half broad, and four inches three quarters high*, formed of very thin sheets of copper. This receiver, which well deserves the name, already celebrated, of *calorimeter*, is furnished with a long neck, near one of its extremities, three quarters of an inch in diameter, and three inches high, intended to receive and support a mercurial thermometer of a particular shape. The receiver has also another neck, an inch in diameter and the same in height, situate in the centre of its upper part, and closed by a cork.

A calorimeter.

Worm of a new form.

Within this receiver, two lines above its flat bottom, is a particular kind of worm, receiving all the products of the combustion

* French measure.

combustion of the inflamable substances burned in the experiments; and transmitting the heat manifested in this combustion to a considerable body of water, which is in the receiver.

This worm, which is made of thin copper, occupies and covers the whole bottom of the receiver, yet without touching either its bottom or its sides. It is a flat tube, an inch and half broad at one end, and an inch at the other; and half an inch thick throughout. It is bent horizontally, so as to pass three times from one end of the receiver to the other; and is supported in its place, two lines above the bottom of the receiver, by several little feet.

The aperture, that forms the mouth of the worm, is a circular hole in its bottom, near its broadest end. Into this hole is soldered a perpendicular tube, an inch in length and an inch in diameter, reaching within the worm to the height of a quarter of an inch above its bottom.

This tube passes through a circular hole in the bottom of the receiver, to which also it is soldered. Its lower aperture is seven lines below the bottom of the receiver; and through this the products of the combustion enter into the worm.

The other extremity of the worm passes horizontally through the perpendicular end of the receiver, opposite to that near which the products of the combustion enter the worm.

The worm, before it passes through the end of the receiver, is fashioned into the shape of a round pipe, half an inch in diameter; and an inch in length of this pipe is seen without the receiver. This piece is made to fit tight into another similar tube, belonging to the worm of another receiver, which I call the *secondary receiver*; the purpose of which is to receive the heat, that might still be found in the products of combustion, after they have passed through the worm of the principal receiver. Secondary receiver.

To support these two receivers in the air, so as not to touch the table that supports them, each of them is fixed in a frame of dry linden wood, made of rods an inch square. Round the bottom of each receiver is a copper rim, three lines deep, which is fastened by a row of very small nails to the wooden frame. The body of the receiver itself enters about a line into the frame, to which it is very accurately fitted. Mode of supporting them.

The

Flatness of the worm essential.

The flat form of the worm is essential to the perfection of the apparatus; as is evident, when its purpose is considered.

All the products of the combustion being elastic fluids, and consequently substances incapable of communicating their heat, but by proceeding particle after particle to deposit it on the suface of the cold and fixed body intended to receive it, it was indispensable so to construct the apparatus, that the hot fluids should of necessity be spread *beneath* and *against* a large flat surface, placed horizontally, and always cold.

Before I employed horizontal worms made of flat tubes, I had more than once tried those of the common form: but they never answered my purpose otherwise than so imperfectly, that I could never make any account of the experiments, in which they were employed. There is no doubt but the shape I have adopted for the worm of my calorimeter would be very advantageous for every kind of apparatus for distillation.

This shape advantageous for a common still.

Shape of the thermometer.

One thing very important in the construction of my apparatus is the shape of the thermometer, which I employ to measure the temperature of the water in the receiver. This thermometer, which I made myself; and which, after having undergone every kind of trial, has always appeared good; is a mercurial thermometer, divided according to Fahrenheit's scale. It is one of four, all similar, that I employed, at Munich, in the winter of 1802, in my experiments on the refrigeration of liquids enclosed in vessels.

The reservoir of this thermometer is cylindrical, about two lines in diameter only, and four inches high: and as the water in my calorimeter is four inches deep, this thermometer always indicates the mean temperature of the fluid, whatever may be the temperature of its different strata.

To measure the heat of a fluid the bulb of the thermometer should extend from bottom to top.

In my various inquiries concerning heat, I have had frequent opportunities of seeing the importance of this precaution; and I cannot conceive how any one can expect to avoid great mistakes in measuring the temperature of liquids heated or cooled, if we do not attend to this.

this. For my own part, I confess, I pay little regard to the experiments of which I am told, when I know they are so negligently made; and assuredly I shall never waste my time, in attempting to build theories on their results.

Complete combustion requisite.

In using the apparatus I have described, several precautions are necessary. In the first place it is obvious, that, when the object is to ascertain the quantity of heat developed in the combustion of any inflammable substance, it is indispensably necessary, so to arrange matters that *the combustion shall be complete.* I have thought, that it might be so considered, whenever the substance burned leaves no residuum, and burns with a clear flame, without smoke or smell.

Smell of the burning substance a proof it is imperfect.

The least smell, particularly that peculiar to the inflammable substance burned, is a certain indication, that the combustion is imperfect.

I had long sought, before I was able to find to my satisfaction, a mode of burning very volatile liquids, such as alcohol and ether: but I have at length discovered it, as will soon appear. I have frequently succeeded in burning highly rectified sulphuric ether, without the least smell of ether being diffused through the room; and it was in these instances alone, that I considered the experiments as accurate.

Method of burning wood completely.

As to wood I have found a very simple method of burning it completely, without the least appearance of smoke or smell. I got a joiner to plane me shavings about half an inch wide, a tenth of a line thick, and six inches long: and holding these in the hand or with pliers, elevated at an angle of 45° or thereabout, and with the edges perpendicular, they burned like a match, with a very clear flame.

The slip of wood that burns being very thin, and placed between two flat flames, which press on it closely, it is exposed to the action of so strong a heat, that it burns perfectly and entirely.

If the shavings employed be too thick, a portion of the charcoal of the wood remains; particularly if it be oak, or any other wood of slow and difficult combustion: and in this case the experiments are defective. But if the shavings

be

be sufficiently thin, and well dried, I have found, that any kind of wood may be burned completely.

Management of candles and lamps.

In burning candles, wax tapers, or fat oils in lamps, the only precautions necessary are so to arrange the wick, as to yield no smoke; to place the flame properly in the aperture of the worm; and to surround the apparatus on all sides by screens, to prevent the flame from being deranged by the wind.

Source of errour from the air cooling the receiver,

In these experiments there is one source of errour, too obvious to escape the most superficial observer, and to which it was important to attend. While the calorimeter is warmed by the heat developed in the combustion of the inflammable substance, which is burning at the aperture of the worm, it is continually cooled by the ambient air, that surrounds it on all sides. It would be possible, no doubt, by calculations founded on a knowledge of the law of refrigeration of the receiver, which might be found by separate experiments, to ascertain the quantity of the effect produced by the refrigeration in question; and this even with a certain degree of precision: but it would have been impossible by this method, or by any other known, to calculate the effects of another cause of errour, less obvious perhaps, but certainly more weighty, than that of the refrigeration of the external surface of the receiver.

and from the nitrogen carried into it.

The nitrogen, which is mixed with the oxigen of the atmospheric air, is necessarily carried into the worm with the proper products of the combustion; and without a precaution, which it occurred to me to employ to prevent the effects of this cause of errour, by making a compensation for them, all the experiments would have been of no value.

Fortunately the method I employed to obviate the effects of this cause of errour was sufficient, to prevent at the same time those, that might have arisen from the cooling of the outer surface of the receiver.

Method of obviating both.

As the receiver is cooled, whether by the atmospheric air in contact with its external surface, or by the nitrogen and other gasses traversing the worm with the products of combustion, only so far as the worm is hotter than the surrounding

surrounding air; while on the contrary it is heated by these elastic fluids, whenever it is at a lower temperature than they are: by arranging matters so, that the temperature of the water in the receiver shall be a certain number of degrees, 5° for instance, below the temperature of the air at the beginning of the experiment; and putting an end to the experiment, as soon as the water in the receiver has acquired a temperature precisely the same number of degrees higher than the air; the receiver will be heated by the air during half the time of continuance of the experiment, and cooled by it during the other half: so that the calorific and frigorific effects of the air on the apparatus will counterbalance each other, and produce no perceptible effect on the results of the experiments; consequently they will require no correction.

Better to avoid or compensate errour, than correct it by calculation.

When we are making experiments to elucidate natural phenomena, it is always more satisfactory to avoid errours, or to compensate them, than to trust to calculation for appreciating their effects.

A small range of the thermometer used.

As the law of the variation of the specific heat of water at different temperatures is not known, and as we have but an imperfect knowledge of the true measure of the intervals of temperature marked by the divisions of our thermometers, to prevent the effects, that our uncertainty on these points would have on the subject of inquiry, I took care to make my experiments in a room, where the temperature varied very little, and to confine them to a few degrees of elevation of the temperature of the water in the receiver.

Other experiments.

It is true, I made some experiments in a room where the air was much colder, and in which I employed ice instead of water to fill the receiver; but these experiments were for a particular purpose, and are not classed with the others. Besides, they never afforded such uniform and satisfactory results, as those made under other circumstances.

Freezing of aqueous vapour

It has been fully proved, not only by the results of my experiments, but by the experiments of others also, that the vapour of water in contact with ice frequently freezes, while this same ice is melting by the heat, or that its thaw appears fully established.

To

Experiment to ascertain the perfection of the apparatus.

To give an idea of the reliance that may be placed on the results of the experiments made with the new apparatus I have just described, I will introduce here the particulars of an experiment, made purposely to discover its degree of perfection.

Having filled two receivers, properly connected with each other, with water at the temperature of the air of the room, 55° F., I burned a wax taper under the mouth of the principal receiver, so that all the products of the combustion passed through the worm of the secondary receiver, after having traversed that of the principal. Each of the receivers contained 2371 gr. [36621·5 grs.] of water.

The following are the results of the experiment.

Time of the observation.			Temperature of the water	
Hours	Min.	Sec.	in the principal receiver.	in the secondary receiver.
9	37		55°	55°
	49	42	65	55
	56	15	70	55
10	2	52	75	$55\frac{1}{8}$
	9	32	80	$55\frac{3}{8}$
	16	34	85	$55\frac{5}{8}$
	23	54	90	$55\frac{3}{4}$
	27			56
	31	40	95	$56\frac{1}{8}$
	39	35	100	$56\frac{3}{8}$
	47	40	105	$56\frac{3}{4}$

The secondary receiver not used in the following experiments as unnecessary.

From the results of this experiment it appears, that the water in the secondary receiver did not begin to be heated perceptibly, till that in the principal receiver had been heated 15° or 20°: and, as I had intended from the beginning never to continue an experiment longer than was necessary to raise the temperature of the water in the principal receiver 10° or 12° F.; it may be supposed, that, as soon as I found by this experiment how little heat remained in the products of combustion after they had passed through the worm of the principal receiver, I relinquished my original design of operating with the two receivers joined together. As it was evident, from the above results, that the second receiver could never be

be sensibly affected; or indicate any thing except the confidence I might place in the indications of the first, I resolved to dispense with the trouble of using it.

It may be seen by the description I have given of this apparatus, that it may be used very conveniently for ascertaining the specific heat of gasses; as well as that made apparent in the condensation of vapours; and generally in all researches, where the quantity of heat communicated by an elastic fluid in cooling is to be measured. And as it would be extremely easy, by very simple means, to separate completely the products of the vapours condensed in the worm from the gasses, that pass through it without being condensed, I cannot avoid hoping, that this apparatus will become useful as an instrument to be employed in chemical analyses. This however would only be an extension of the method already employed with so much success by Mr. de Saussure, and by Messrs. Gay-Lussac and Thenard. The apparatus applicable to ascertain the specific heat of gasses.

As soon as my apparatus was finished, I was eager to see what quantity of heat I should find in the combustion of wax, and in that of olive oil, that I might afterward compare the results of my experiments with those of Mr. Lavoisier's: and, as I have the most implicit reliance on every thing published by that excellent man, I sincerely wished to find in this comparison a proof of the accuracy of my method, and at the same time a confirmation of the estimates of Mr. Lavoisier. Experiments to be compared with Lavoisier's.

Sect. I. *Experiments made with white wax.*

The air of the room being at the temperature of 61° F., 2781 grammes of water, of the temperature of 56° F., were put into the receiver of the calorimeter, (including the quantity of this liquor that represents the specific heat of the instrument); and, a lighted wax taper having been properly placed at the entrance of the worm, the calorimeter was heated for 13 min. 26 sec.; when, the thermometer announcing that the water had acquired the temperature of 66° F., the taper was extinguished. Combustion of white wax.

As I took care to weigh the taper before it was lighted, I found by weighing it at the end of the experiment, that 1·63gr. of wax had been burned.

To express the results of this experiment so as to render them obvious, and at the same time easy to be compared with the results of other similar experiments, we will see how much water of the temperature of melting ice would have been made to boil, at the mean pressure of the atmosphere, by the heat made apparent in the combustion of the 1·63gr. of wax burned.

The distance on Fahrenheit's scale between the temperature of melting ice and boiling water being 180°, if the burning of 1·63gr. of wax were requisite to raise the temperature of the water in the calorimeter 10°, the burning of 29·34gr. would have been necessary, to raise it 180°: and, if 29·34gr. of wax could furnish by combustion sufficient heat to raise the temperature of 2781gr. 180°, a gramme of this inflammable substance must furnish enough, to heat 94·785gr. of water to the same point.

Quantity of water heated 180° by it.

Consequently one pound of white wax, or wax taper, should furnish in burning sufficient heat, to raise 94·785lbs. of water from the temperature of melting ice to the boiling point.

Quantity of ice melted.

To find how many pounds of ice this quantity of heat would melt, we have only to add to the number of pounds of water at the temperature of melting ice it would cause to boil the third part of this number, and the sum would express the weight of the ice in pounds.

This, then, for white wax is ······94·785
+ 31·595

= 126.380lbs. of ice melted for one pound of the wax burned.

Two other experiments with wax.

Before I compare the result of this experiment with that of an experiment made with the same substance by Mr. Lavoisier, I will give an account of two other experiments I made with wax, as the reader will undoubtedly be struck with the uniformity of their results. This is so remarkable, that I should scarcely venture to publish them, had I not proofs, that all my experiments were actually made and minuted down, before I began my calculation of their results; and were I not assured, that any person, who will follow my method, using the same apparatus, will find the same results on repeating my experiments.

As the mode of operating in making these experiments must now be well known, I may suppress the particulars in what follows without inconvenience, and give only the results of the experiments.

I will begin with three experiments made with white wax; and to render them more easy to compare, I will give them together in a tabular form.

Tabulated results.

Results of three experiments on the burning of white wax, showing the quantity of water that would be heated 180°, or of ice that would be melted, by one pound weight of it.

No. of the expt.	Quantity of wax burned.	Time employed in burning.		Quantity of water heated.	Elevation of its temperature.	Temper. of the water at the beginning.	Temper. of the water at the end of the exp	Temperature of the air.	Results: lbs of water heated 180°	Results: lbs of ice melted.
	Grammes	Min.	Sec.	Grammes.	Deg. Fahr.	Deg. Fahr.	Deg. Fahr.	Deg. Fahr.	lbs.	lbs.
1	1·63	13	24	2781	10°	56°	66°	61°	94·785	126·38
2	2·36	19	30		14·5	51	65·5	58	94·926	126·608
3	2·17	18	15		13·25	51·75	65	58	94·337	125·783

Mean of the three experiments.

If we take the mean term between the results of these experiments, we shall find, that the quantity of heat developed in the combustion of wax is such, that one pound of this substance is sufficient, to raise 94·682lbs of water from the temperature of melting ice to the boiling point; and consequently, that it should melt 126·242lbs of ice.

Results of Lavoisier's.

According to the experiments of Mr. Lavoisier, the heat developed in the combustion of one pound of white wax was sufficient to melt 133·166lbs of ice.

The difference small:

The difference between the results of our experiments with this substance is not very great; and, if those of Mr. Lavoisier were made at a time, when the temperature of the air was only a few degrees higher than that of melting ice (which I have no means of ascertaining), the quantity of nitrogen, that must have entered into the calorimeter with the oxigen employed to support the combustion, would have been so great as to account sufficiently for the difference. But the very great difference between the results of our experiments made with olive oil proves, that one or other of our processes must have been defective.

but great in combustion of oil.

Result of the author's experiments:

The mean result of several experiments made with olive oil gave me for the measure of the quantity of heat developed in the combustion of one pound of this substance 90·439lbs of water heated 180° F; or 120lbs of ice melted, neglecting the fraction.

and of Lavoisier's.

In the experiments of Mr. Lavoisier more than 148lbs of ice were melted by the heat, that appeared to result from the combustion of one pound of this oil.

The latter suspected to be great by himself.

It is true, that this result was considered by that eminent philosopher himself as too great to be capable of explanation; and he added, with that modesty which rendered him so engaging and so respectable: "We shall probably find ourselves under the necessity of making corrections, perhaps pretty considerable ones, in most of the results I have given: but I did not think this a sufficient reason, to delay affording their assistance to those, who might intend to pursue the same object."

Rape oil purified by sulphuric acid compared with olive oil.

As it appears very probable, that all the fat oils, when perfectly pure, are composed of the same principles, I was curious to see whether rape oil, purified by sulphuric acid, would not

not afford more heat in its combustion than olive oil, when burned in its natural state. The result of three experiments showed me, that rape oil thus purified does in fact yield more heat than olive oil. The difference is indeed pretty considerable, and more than I could have suspected.

The combust. of 1lb of purified rape oil gave
93·073 of water heated 180°.
——————— olive oil gave 90·439.

Chemists may tell us, whether the quantity of incombustible matter separated from rape oil in purifying it be sufficient, or not, to account for this difference.

Comparison of oil and wax.

On comparing the results of the experiments made with white wax and those with the purified oil, it appears, that equal weights of these substances afford nearly equal quantities of heat in their combustion: and as in fact this ought to be the case, from the quantities of combustible matter they contain, the result tends to strengthen our confidence in this method of measuring the heat developed in combustion.

It was with the combustion of
1lb of white wax 94·682lbs of water heated 180°.
1lb of purified oil 93·073lbs.

Combustion of hidrogen and carbon the object of research.

As the object I had chiefly in view in this series of experiments was to ascertain the quantities of heat developed in the combustion of pure hidrogen and carbon, in order to render this method useful in some chemical analyses, I examined particularly those inflammable substances, that had been analysed with most care.

This has been attempted directly.

Several attempts have been made to ascertain these interesting questions by direct experiments, in burning pure hidrogen, or pure hidrogen and carbon; but the results of these researches have varied so much, that they cannot be relied on.

Hidrogen rated higher by Crawford than by Lavoisier:

According to Crawford, the heat developed in the combustion of one pound of hidrogen gas is sufficient to raise the temperature of 410lbs. of water 180° F. But the estimation of Mr. Lavoisier is much lower. According to him this heat would raise only 221·69 lbs of water the same number of degrees.

On

carbon the contrary.

On the other hand Mr. Lavoisier estimates the quantity of heat developed in the combustion of charcoal much higher than Dr. Crawford. I have many reasons to believe, that they both estimate it too high: and, if this opinion be confirmed, we must estimate the heat developed in the combustion of hidrogen a little higher even than Crawford has done, to be able to account for that manifested in my experiments.

Perhaps both rate this too high,

hidrogen too low.

Charcoal according to the author,

From several experiments, which I made five years ago, it appeared to me, that one pound of charcoal, dried as much as possible before it was weighed by heating it red hot in a crucible, was not capable of raising more than from 52 to 54 lbs. of water from the temperature of melting ice to a boiling heat.

Crawford, and Lavoisier.

According to Crawford this heat should suffice to boil 57·606 lbs.; and according to Lavoisier, 72·375 lbs.

We shall see how these estimates agree with the results of my experiments.

Results, from wax compared with those calculated from its component parts:

As the experiments made with wax yielded very uniform results, and as the analysis of this substance has been made with great care, I shall examine how the quantities of hidrogen and carbon in this substance agree with the quantity of heat, that it afforded me in combustion.

According to the analysis of Messrs. Gay-Lussac and Thenard, a pound of this substance contain

Carbon	0·8179
Free hidrogen	0·1191

If we adopt the calculations of Dr. Crawford, both for the heat furnished by the hidrogen, and that furnished by the carbon, we shall have for the heat that should be furnished by the combustion

according to Crawford,

	lbs of water raised from 32° to 212°
Of 0·1191 lb. of hidrogen, after the ratio of 410 lbs. of water raised from 32° to 212° by burning 1 lb. of hidrogen	48·831
Of 0·8179 lb of carbon, after the ratio of 57·606 lbs. of water raised from 32° to 212° by burning 1 lb. of carbon	47·116
Total of the heat, that ought to be furnished by the quantity of combustible matter (hidrogen and carbon) in 1 lb of white wax	95·947 lbs

Quantity

Quantity of heat furnished by 1 lb. of white wax, during its combustion, according to my experiments .... 94·682 lbs — to the author's experiments,

If we adopt the calculations of Mr. Lavoisier for the heat furnished by carbon and hidrogen in their combustion, we shall have for the heat that ought to be furnished by the burning

Of 0·8179 lb. of carbon, after the ratio of 72·375 lbs. of water heated 180° by 1 lb..... 59·195 lbs — to Lavoisier.
Of 0·1191 lb. of hidrogen, after the ratio of 221·69 lbs. of water heated 180° by 1 lb. .... 26·403
Total of the heat that ought to be furnished by the combustible matter in 1 lb. of white wax .... 85·598 lbs

From the results of these calculations it appears, that the estimations of Dr. Crawford agree much better with the experiments than those of Mr. Lavoisier. — Crawford's nearer than Lavoisier's.

Let us see how the results of the experiments made with fat oils agree with the estimations of these gentlemen. — Comparison of fat oils.

According to the analysis of Messrs. Gay-Lussac and Thenard a pound of olive oil contains

Carbon ....0·7721 lb — Results,
Free hidrogen ....0·1208

According to the calculations of Mr. Lavoisier we have, — according to Lavoisier,
For 0·7721 lb. of carbon ··55·881 lbs. of water heated 180°
0·1208 lb. of hidrogen 26·780

Total 82·661

According to the calculations of Dr. Crawford it is — to Crawford,
For 0·7721 lb. of carbon ··44·478 lbs. of water heated 180°
0·1208 lb. of hidrogen 49·528

Total 94·006

According to the experiments 1 lb. of purified rape oil furnished heat sufficient to raise 93·073 lbs. of water 180°; and 1 lb. of olive oil enough to heat 90·439 lbs. — to experiment.

From all these comparisons it follows, that the estimations of Dr. Crawford agree much better than those of Mr. Lavoisier with the results of my experiments. — Crawford's still nearest.

SECT.

SECT. II. *Experiments made with spirit of wine, alcohol, and sulphuric ether.*

Experiments with inflammable liquids.

As the component parts of these inflammable liquids may be considered as well ascertained by the results of the excellent investigation of Mr. de Saussure*, I undertook to examine them for the second time, in order to discover what quantities of heat are developed in their combustion. I had begun this undertaking five years ago; but, after having made a considerable number of experiments, I desisted from it, on account of the great difficulties that occurred. As soon, however, as I had found means of rendering my apparatus more perfect, I formed the project of recommencing it.

Difficulties in them,

and even danger.

Before I enter into the particulars of my experiments, I must say a few words respecting the difficulties that occurred to me, even after I had my new apparatus; and of the means I employed to surmount them. I even found myself exposed to dangers, which it is necessary for me to mention as a caution to those, who may undertake the same inquiry.

Evaporation of the liquid.

Attempt to prevent it.

When I made the experiments with highly rectified alcohol, and more particularly with ether, I found it very difficult to prevent a portion of these volatile liquids from escaping in the state of vapour from the bulk of them remaining in the lamp. I procured a small lamp, resembling in shape a small round snuffbox, with a nozzle rising from the centre of the circular plate, which closed it atop; and on this plate was fixed a small pan, to hold cold water, for keeping the nozzle cool, and preventing the heat from being communicated to the body of the lamp. But this precaution was not sufficient, when I burned ether, as I found to my cost: for though the pan was twice the diameter of the lamp, and filled with very cold water, the water was so heated in a few minutes, that an explosion took place from vapour of ether kindling in the air with a flame that rose to the ceiling. Indeed it was near setting the house on fire.

Dangerous explosion.

Another lamp constructed.

Warned by this accident I procured a new lamp, much smaller than the former, being only an inch in diameter and three quarters of an inch deep; and its nozzle, which was only two lines in diameter, was three quarters of an inch high.

* See Journal, vol. xxi, pgs. 222, 259, 321.

high. To keep this small lamp cool while burning, it was placed in a small pan, and kept constantly immersed in a mixture of water and pounded ice to within a quarter of an inch of the extremity of the nozzle. These precautions were sufficient to prevent any explosion, though not the evaporation either of the ether or of the alcohol. This fact I learned from observing, that, as often as I made two consecutive experiments without filling the lamp afresh, the alcohol constantly appeared weaker in the second experiment than in the first.

This prevented explosion, but not evaporation.

The cause of this phenomenon was not difficult to discover. The most volatile, and consequently the most combustible parts of this liquid, being diffused in vapour in the interior of the lamp, found means of escaping through the nozzle with the part of the liquid that traversed the match, leaving the alcohol, that remained in the lamp, perceptibly weakened.

To remedy this imperfection I constructed a third lamp, which I now submit to the inspection of the class. It is made of copper, and has the shape of a small cylindrical vase, an inch and half in diameter, and three inches high, swelling out a little atop, and closed hermetically by a copper stopple, which, being ground with emery, fits tight into the neck of the vase. Through the centre of this stopple passes a small perpendicular hole, which can be shut completely, or left a little open, as may be required, by means of a small screw carrying a copper collar.

A third lamp that succeeded completely.

A small tube, about an eighth of an inch in diameter and two inches and half long, proceeds horizontally from the side of the vase very near the bottom. At the distance of an inch and four lines from the vase this tube is bent at a right angle, rising upwards perpendicularly to form the nozzle of the lamp.

This little tube is every where very thin, except at its upper extremity, where it is made thicker, to admit of being shaped so as to fit tight into a very small cylindrical extinguisher, 5 lines high by 3·5 in diameter; intended to close the nozzle hermetically without touching or deranging the wick, the moment the lamp ceases to burn; and to keep it constantly closed, when the lamp is not lighted.

Without

Without this precaution; in experiments made with ether, so large a quantity of this volatile liquid would evaporate through the nozzle of the lamp while weighing, that it would be impossible to ascertain the quantity burned.

The nozzle of the lamp is steadied by two pieces of wire, proceeding from it horizontally, and soldered to the body of the lamp.

To keep this lamp constantly cold, as well as the liquid it contains, it is placed in a small pan, and covered completely, except the extremity of its nozzle and that of its neck, with a mixture of pounded ice and water.

Caution.

When the lamp is weighed, it is taken out of the pan, and well wiped with a dry cloth, before it is put into the scale.

When the lamp is kindled, the operator must not forget, after it has burned two or three minutes, to open the screw that closes its stopple a little, though but *very little*, otherwise it might go out.

As the little horizontal tube, by which the liquid that is burned passes from the reservoir of the lamp to its nozzle, is always filled with liquid, so that it can have no communication with the vapour diffused in the upper part of the reservoir, this vapour cannot escape by the nozzle of the lamp, as it did before I thought of this method of preventing it.

Apology for minuteness.

If I have been very minute in my description of this lamp, it was because I thought it necessary to spare those, who might be disposed to repeat my experiments or make similar ones, all the difficulties I had to surmount, before I found the means of having under command the combustion of very volatile inflammable liquids.

As the apparatus I have employed has now been described, it will be easy to follow the steps of my experiments, and to appreciate their results. I will endeavour to describe them clearly, but also as briefly as possible.

Spirit employed in the experiments.

Having procured a stock of spirit of wine of the shops, and of alcohol of different degrees of purity, I ascertained with the greatest care their specific gravities at the temperature of 60° F., taking that of water at the same temperature as 100000. I chose this temperature, that I might afterward the more easily ascertain the quantities of water, that each ought to contain, according to the tables constructed from the experiments of Mr. Lowitz.

The following table will show the specific gravity of each, and the quantity of pure alcohol of Lowitz and of water contained in it.

Liquid.	Spec. grav. at 60° F.	Composition. Pure alcohol of Lowitz.	Water.
Alcohol of 42°	817624	0·9179	0·0821
Alcohol of the shops	847140	0·8057	0·1943
Spirit of wine of 33°	853240	0·7788	0·2212

The following are the results of the experiments made to ascertain the quantities of heat, which these liquids furnished in burning. Results

In three experiments made with the spirit of wine the quantities of heat manifested were, with the weakest spirit:

in the 1st, 53·260 }
2d, 51·727 } lbs of water raised from the temperature of melting ice to that of ebullition.
3d, 52·604 }

The mean result is 52·604 lbs*.

As a pound of this liquid contained but 0·7788 of the alcohol considered by Lowitz as pure; the other part, = 0·2212, being only water, which does not burn; to find how much water would be raised from the temperature of melting ice to that of ebullition by a pound of the pure alcohol of Lowitz, we have only to divide the quantity, that is the measure of the mean heat developed in the experiments with the spirit of wine by the fraction, that expresses the quantity of alcohol in a pound of this liquid.

Thus we have $\frac{52·604}{0·7788} = 67·545$ lbs, the measure of the heat developed in the combustion of one pound of pure alcohol of Lowitz, according to the mean result of the experiments made with spirit of wine.

In two experiments made with the alcohol of the shops, the mean result was 54·218 lbs: and, as this contained with the next:

* As the mean of the three preceding numbers would be 52·530, there is evidently some mistake; and the last number of the three being the same with the mean given, it is probable, one of these is an errour of the transcriber. But, as the number 52·604 is employed as the mean in the calculation in the next paragraph, it may be presumed, that the result of the third experiment should have been 52·825. C.

0·8057lb

0·8057 lb. of pure alcohol, we have for the measure of the heat developed in the combustion of 1lb of pure alcohol $\frac{54{\cdot}218}{0{\cdot}8057} = 67{\cdot}294$ lbs of water heated 180° F.

with the strongest.

Of three experiments made with the alcohol at 42° the mean result was 61·952 lbs. of water heated 180° F. by the heat developed in the combustion of one pound of this liquid.

Hence 1lb of pure alcohol should furnish heat enough in burning to raise 67·57 lbs of water 180° F.; for $\frac{61{\cdot}952}{0{\cdot}9179} =$ 67·57*.

Mean for pure alcohol:

Taking the mean between the results of these eight experiments with three alcoholic liquors, we shall have for the measure of the heat developed in the combustion of one pound of pure alcohol of Lowitz 67·47† lbs of water raised from the temperature of melting ice to that of ebullition.

compared with its component parts.

It will be extremely interesting, no doubt, to know whether this quantity of heat agree with the quantities of combustible matter (carbon and hidrogen) in alcohol. We will see.

According to the analysis of Mr. de Saussure, 1lb of the alcohol of Lowitz contains

Carbon	0·4282
Free hidrogen	0·1018
Water	0·4700
	1·

The calculation from Crawford,

Now according to the calculations of Dr. Crawford we shall have for the measure of the heat developed in the combustion of

0·4282 lb of carbon	24·667 lbs of water heated 180° F.
0·1018 lb of hidrogen	41·738
Total	66·405

and the re-

The experiments gave us 67·47†

* If the mean result were as given above, which I have no means of knowing, as the results of the experiments are omitted, this should be 67·493. C.

† If the correction in the preceding note were to be made, this should be 67·444. C.

It

It is rare in a research of such delicacy to find the results of experiment agree so perfectly with those of calculation. Result of the experiment very nearly agree.

In the conclusion of this paper I shall have the honour of giving the class an account of the results of a considerable number of experiments, which I have just made to ascertain the quantities of heat developed in the combustion of sulphuric ether, naphtha, suet, and several kinds of wood; as well as that manifested in the condensation of the vapours of water, of alcohol, and of ether. Farther experiments with other substances

These experiments are all finished, and I have made considerable progress in the paper, in which I purpose to give an account of them here. I flatter myself, that the class will do me the honour to listen to it with its usual indulgence, at an early meeting*. promised.

V.

Remarks on the Experiment of Dr. Bostock *and Dr.* Traill. *In a Letter from a Correspondent.*

To W. NICHOLSON, Esq.

SIR,

THE experiments of Drs. Bostock and Traill cannot, I think, be considered as decisive in proving, that water is produced in the combination of dry muriatic and ammoniacal gas. The mode adopted in drying the ammoniacal gas by these gentlemen is not effective. A lump of quicklime introduced into a jar of this gas would not absorb the whole of the combined moisture. Lime is not so greedy of moisture as some other substances, and in a mass would be disposed to take up but little: had it been introduced immediately from the fire in the state of powder, it might have been more effectual. To deprive gas of the moisture it contains, the best method has been found to be to pass Experiment of Dr. Bostock and Dr. Traill not decisive, a lump of lime being insufficient to dry muriatic gas. Hot lime in powder better;

* This the Count has promised to transmit for insertion in the Journal, as soon as he can find an opportunity.

it

but hot muriate of lime best.

it repeatedly through muriate of lime in coarse powder previously heated; or to agitate it for some time in contact with this salt in a dry vessel.

If Drs. Bostock and Traill will take the trouble of repeating their experiment with this precaution, they will, I believe, find the result to be as A. B. C. has stated it.

I am, Sir,

Your most humble servant,

BRISTOL, D. E. F.
11th of May, 1812.

VI.

Method of preparing a cheap and durable Stucco, or Plaster, for outside or inside Walls: by H. W. WAY, Esq. *of Bridport Harbour**.

SIR,

Stucco for houses exposed to bad weather.

IN consequence of your expressing an opinion, that a general knowledge of my method of preparing a stucco, or plaster, for outside walls of houses much exposed to sea breezes or bad weather, would be of service to the public, I have enclosed an account of the process; and I will with pleasure furnish any farther particulars of this business for the Society of Arts, or permit any gentleman to examine it, who may wish for more information on the subject. You know the situation of my house, which is greatly exposed to the spray of the sea and bad weather; and I can truly add, that by means of this stucco it is perfectly free from damp, and the plaster remains compact and durable.

I remain, Sir,

Your obedient humble servant.

BRIDPORT HARBOUR, H. B. WAY.
Oct. 12, 1810.

* Trans. of the Soc. of Arts, vol. XXIX, p. 73. The silver medal was voted to Mr. Way; and specimens of his stucco, and of the sand from which it was made, are preserved in the Society's repository.

To

To make a strong Stucco, or Mortar.

Three parts Bridport Harbour sand to one of lime, both finely sifted and mixed with lime-water; if used as stucco, the first coat to be laid on half the thickness of a crown-piece; let it remain two days, then with a painter's brush wash it over with strong lime water, and lay on the second coat of the same thickness. Method of making it.

1805, March 25.—Measured a coal half-bushel of Beamister lime*, and put it into a hogshead of water, to make the lime-water.—Measured two coal half-bushels more of the lime, slaked and sifted it, it then measured three half-bushels, to which were added nine coal half-bushels of Bridport Harbour sand well sifted; I saw it well mixed up with lime-water, and thoroughly worked together; the next day saw it turned, and again mixed up, that it might be well incorporated together.

27th.—This morning had a fine coat of it laid on the west end of my large storehouse at Bridport harbour.

29th.—Had it washed with lime-water, and a second coat laid on.

Cost.

	s.	d.	
One sack and a quarter of lime, at 2s. 6d. -	3	$1\frac{1}{2}$	Expense.
Two men and one boy two days each, fetching and mixing up materials, and laying on; men 2s. 3d. per day, boy 10d. per day, and one pint of ale each per day, 12d. - - -	11	$10\frac{1}{2}$	
	15	0	

N.B.—I suppose the expense rather over than under-rated.

May 11.—This day Thomas Everett measured and examined the work, found it hard and sound, $24\frac{2}{3}$ square yards, a little done to the house, suppose the whole to be twenty-five yards square.

Twenty-five square yards at $7\frac{1}{4}d.$ per square yard, would be 15s. $1\frac{1}{4}d.$

* This appears, from a subsequent part of the paper, to be chalk lime. C.

June

Its durability. June 13, 1806.—Examined the work, which was perfectly sound and free from cracks, nothing having ever peeled off from it. The situation exposed to the weather in the greatest degree.

N. B.—The coal half-bushel above mentioned holds exactly thirteen gallons wine-measure.

H. B. WAY.

SIR,

Farther account of the stucco.

I WAS favoured with yours of the 18th instant, and I now enclose the mason's certificate of the quantity of stucco done with the composition I gave him the particulars of; in addition to which it may be necessary to mention, that the coal half-bushel, with which the ingredients of the composition were measured, (according to the account formerly given), contains exactly thirteen gallons of water, wine measure, and would hold exactly 1 cwt. 1 qr. 7 lb. net of the sand used. The weight of the lime I do not know; and my being able to ascertain exactly the weight of the sand arose from my waggon being employed to carry what was used at Yeovil, and East Coker, from hence; and for what I sent to Yeovil I was paid 1*s.* 9*d.* per cwt. From the sand here succeeding so well, Thomas Everett would not be prevailed on to engage to do any of that sort of work with hill or river sand, to be got on this shore. The work he did for me was all by the day; what he did at Yeovil and East Coker he agreed for at eight pence per yard, of nine feet superficial measure for labour only for the two coats, at four pence per square yard for one coat, all the materials being brought to the spot at his employer's expense, and who also found scaffolding and scaffold ropes. This, I think, is considerably higher than by my calculation of the expense of what I had first done he ought to have charged; but its being done at a distance of twenty miles from where he lives, and in the most busy time of the year for masons work, I suppose must account for it in the first instance; and having once made that price, he would not now work under: but, I believe, for a considerable building, and with sufficient notice, and being allowed 6*d.* per mile in lieu of wages and travelling expenses for himself

Charge for it.

and

and an assistant, out and home, he would go to any part of the kingdom, on being paid 8*d.* per yard for the work. It has been the general received opinion here, that plaster made with sea sand, unless well washed in fresh water, would always be damp; but, on the contrary, I find from what has been done in my dining-parlour and passage, it has been always quite dry, although the whole of the sand with which it has been done has been thrown up by the sea, and must have been always at spring tides covered with sea water: indeed it sometimes happens, that, for months together, there is none to be collected on our shores at this place, that Everett thinks fine enough for the purpose; and as I am now and then applied to for getting it, I have lately, when my horses were at leisure, got a small quantity collected and hauled in for my own use, or, in case of its being wanted, I charge 2*d.* per cwt. for it, where it is deposited. As I design at some future time to make some alteration in the passage done with the stucco in April 1806, I had four pieces taken off, which I tied up separately, each in a piece of brown paper, and had them packed in a box, with a layer of sand between each piece, and at the bottom and top of the box, and directed it for you, and sent it with some goods I shipped on Saturday last to my friend Netlam Giles, Esq. No. 2, New Inn, St. Clement's. I have requested of him, that he will have the goodnes on its arrival to forward it to you. The vessel it goes by is the sloop Mary Ann, John Anning, master, bound to Dounes Wharf, Hermitage, Wapping. It is possible, that the pieces of stucco sent may imbibe some of the saline particles of the sand it was packed up in; but this did not occur to me at the time or they should have been packed in saw-dust; as they were perfectly dry when packed, so much so as, when struck upon with the knuckle, to give a sound similar to what an earthen vessel would do if dry and not cracked. Should there be any further information requisite, on your letting me know, it shall immediately be sent you. It had almost escaped me to say, that the small quantity of six yards, done last October with stone lime for trial, was done from your intimating

Sea sand not liable to get damp.

Hardness of the stucco.

Stone lime appears to answer the purpose.

intimating to me, when I had the pleasure of seeing you in Dorsetshire, that stone lime was likely to answer; but it would I think look better if white washed; the difference in point of expense is materially in favour of the stone lime. The cost of my waggon-load of it at the kiln, about a mile hence, would be only 10*s.*, whereas about the same quantity of chalk lime at the kiln, full eight miles from hence, would cost 1*l.* 4*s.*, and I cannot get any chalk-lime nearer. I have only now to add, that I am, very respectfully,

Sir,

Your obedient humble servant,

BRIDPORT HARBOUR,
April 22d 1811.

H. B. WAY.

Certificate.

Account of stucco work done.

I hereby certify, that Mr. H. B. Way, merchant, of Bridport Harbour, in the county of Dorset, in the month of March 1805, gave me the necessary directions for making a strong cheap stucco or plaster, which was composed of one part chalk lime, and three equal parts of fine sand, collected on the seashore, near Bridport Harbour, the whole of which was mixed up to a proper consistence with a strong lime water; and I have since that time done the annexed work with the said composition.

For Mr. H. B. Way, at Bridport-Harbour.

	Yds.	Yds. flat Msr.
1805. March.—On the outside of a warehouse wall, part rough stone and part brick	25	
1806. April.—On the inside walls of a passage in his dwelling-house, on rough stone.	10	
Oct. & Nov.—On the inside rough stone walls of two cellars.	224	
One coat on the ceilings of the said cellars	228	
N. B. The first coat on the ceilings was common hair mortar		
1807. April & May.—The whole of the outside of hisdwelling-house, rough stone walls	335	

	Yds.	Yds. flat Msre.
August.—On one side wall of the dining-room in brick; this stucco was rubbed down quite smooth, and has since been painted with oil colours	13	
1810. Oct. 10.—On a rough stone wall of a warehouse directly fronting the sea, and not two hundred yards from it, with common stone lime, such as is used for manure in this quarter, by way of trial	6	
		821

1811. April.—At Mr. H. B. Way's request, I have this day carefully examined the whole of the above work, and I find it sound and good, and by his directions, four pieces of the stucco were taken off from the passage wall, (which was laid on April 1806), and packed in the same sort of sand as is used in the composition, and sent by him directed for the Secretary of the Society of Arts, Manufactures, and Commerce, London.... ....

For Peter Daniel, Esq. of Yeovil, Somersetshire.

1808. May & June.—On the outside brick-walls of his dwelling-house there.	430	
1810. May & June.—On the outside brick-walls of other dwelling-houses there..................	480	
		910

For W. Hellyer, Esq. of East Coker, near Yeovil.

1809. June.—On the outside brick and rough stone walls of his dwelling-house, at that place. ..		1040

For the Rev. Joseph Fawcett, of Yeovil.

1810. June.—On the outside rough stone walls of his dwelling-house there		212
N. B. Mr. Fawcett's house being built the year before, with a view to being stuccoed, the walls were left rough.		Yds. 2983

I hereby certify, that the whole of the foregoing two thousand nine hundred and eighty-three square yards of stucco, were done with the before-mentioned composition, by me and my men under my directions; and I verily believe it is

 the

the cheapest stucco known, and that it will prove very durable, both without doors and within, and that it has given entire satisfaction to the gentlemen who have tried it; and I am now engaged, if I can, the ensuing summer, to stucco the outside of one house at Bridport, and another at Yeovil, also the inside of a cottage for labourers that I have just built for Mr. H. B. Way, at Bridport harbour.

THOMAS EVERETT.
Stone Mason, Bricklayer, and Plasterer.

Shipton George, near Bridport,
Dorset, April 22, 1811.
Witness, JAMES BUDDEN.

VII.

Manufacture of Cloth and Cordage from Nettles, by Mr. EDWARD SMITH*.

Cordage and cloth from nettles.

IN page 109 of the 28th volume of the Society's transactions† will be seen a communication from Mr. E. Smith, of Brentwood, on manufacturing a variety of articles from the fibres of the common nettle, for which he has received their silver medal. He has since, with great attention and laudable industry, extended his experiments on this subject, and, during the last session, produced to the society specimens of cloth and cordage made from the nettle, which appear to possess great strength and durability. The society have, therefore, this session, voted to him their silver Isis medal. The following communication was received from him, and specimens of the cordage and cloth, made by him from nettles, are preserved in the Society's repository.

ESTEEMED FRIEND,

Introduction of a new substance of productive labour

I received thy kind favour of the 23d instant, by the contents of which I am much obliged; and being impressed by the consideration of the vast importance the introduction of

* Trans. of Soc. of Arts, vol. XXIX, p. 81.
† See Journal, vol. XXIX, p. 161.

a new

a new substance of productive labour would be of to the community of this manufacturing country, particularly as affording a new source of industry to the increased numerous poor of both sexes, in truth, so operated on my mind, as to create a great unwillingness to suffer any exertions consonant with my limited powers, from total disadvantages, to lie dormant. I am, therefore, very desirous by unremitted endeavours to be instrumental in disseminating the knowledge of, and the means of bringing into use, so valuable a spontaneous production as the common nettle substance, under the sanction and through the medium of the enlightened Society of Arts &c. These considerations, aided by the hope of obtaining their farther approbation, have stimulated me to persevere in my attempts to contribute all in my power towards the advancement of so desirable and beneficial an object; in the expectation, that when all the different fabrics, which that substance is capable of being converted into, are produced, it may have a greater tendency towards encouraging others to embark in a manufactory thereof, than volumes written on the subject. With these sentiments I am induced to trouble thee farther, in requesting thou will be so kind to favour me by laying before the Society the different specimens of manufactory from the nettle substance, which I have at present in readiness, and which will be sent to thee by the Brentwood coach, which inns at the Blue Boar, Aldgate, and I expect will be delivered soon after the receipt of this. The cordage Nos. 1 & 2 is affirmed by the cord-spinner to be of equal strength to that made from hemp. The cloth No. 1 is rough from the loom; No. 2, the same fabric half bleached; and No. 3, which I ordered to be *huckaback*, also is only half bleached for want of sufficient time for the process. The quality of the cloth hath suffered throughout, by the negligence or willfulness of the manufacturer, and is principally owing to the raw material having been only passed through such heckles as are used for the coarse part of the hemp manufactory;—other necessary operations were omitted, in consequence of my instructions not being attended to by the person into whose care it was entrusted. He resides in the country, at a great distance, and his capability and integrity proved greatly inferior to the opinions I had entertained of him;

Fabrics from the nettle.

Cordage.

Cloth.

This injured by the manufacturer.

him; and it now appears his practice is confined to the coarser part of the hemp manufactory. It was my intention to have produced with the above a pair of stockings, manufactured on the principle of cotton, and also a specimen of fine cloth on the same principle, with a view to show the great extent of contrast; but, on application to a cotton spinner, I found the quantity of material I had in a state of preparation suitable was not sufficient for the operations of carding; in consequence I am obliged to postpone my designs till I am enabled to prepare a sufficiency. Greatly desirous of contributing to the accomplishment of the object in view, and sensibly how much the sanction and approbation of the Society would tend to promote it, I hope they will consider my continued exertions worthy their farther attention. Anticipating their approbation, I remain,

Very respectfully,

Thy assured Friend,

EDWARD SMITH.

Brentwood, the 26th of 3d Month, 1811.

VIII.

Account of Herrings cured in the Dutch mode on board British Vessels; by FRANCIS FORTUNE, *Esq.**

Fishing for herrings.

IN the deep sea (which is the principal fishery for herrings) the nets are cast from the busses by sunset, and they drive by them alone expecting the shoals, the approach of which is generally indicated by small quantities of fish; and their arrival by immense flights of sea fowl. The best fishing is with the wind off shore, for, when it blows in a contrary direction, the shoals are broken and dispersed, and the fishery is seldom successful while it continues in that point.

Management of them when caught.

Immediately after the nets are hauled in, (which is often performed with considerable difficulty, by means of a wind-

* Abstracted from the Trans. of the Soc. of Arts, vol. xxix, p. 157. The gold medal, being the premium offered, class 165, for curing British white herrings in the Dutch method, was adjudged to the author.

lass

lass when they are full) the crew begin to gyp the fish, that is, to cut out the gill, which is followed by the float or swim, and divide the large jugular or spiral vein with a knife at the same time, endeavouring to waste as little of the blood as possible;—at this work the men are so expert, that some will gyp fifty in a minute.

Salting.

Immediately after they are gypped, they are put into barrels, commencing with a layer of salt at the bottom, then a tier of fish, each side by side, back downwards, the tail of one touching the head of the other, next a layer of salt, and so alternately until the barrel is filled :—they are thus left, and the blood which issues from the fish, by dissolving the salt, forms a pickle infinitely superior to any other that can be made. The herrings thus drained of their blood occupy less space, and the whole consequently sinks about one third down the barrel, but this sinking is at an end in about three or four days.

Precautions against loss of pickle from leaking.

When these operations are being performed, the sea is often running mountains high; and it is not therefore to be supposed, that the barrels are so well coopered as not sometimes to allow the pickle to leak out; and in order to preserve the fish from being spoiled, which would otherwise happen in such cases, some of the gills and entrails are always put by in barrels with salt, in the same manner as the herrings, and yield a pickle of the same quality; with this pickle those barrels which have leaked are replenished, and the fish sustains no injury. Every operation is performed in the shade, into which the fish are immediately conveyed on their being hauled on board. Each day's fishing is kept separate with the greatest care. The salt used is mixed, and of three different sorts, viz. English, St. Ubes, and Alicant, and each barrel marked with the day of the month on it on which it was filled.

Fish kept in the shade, each day's separate. Salt used.

Advantages of gypping.

The advantages of gypping the herrings are, that the blood, which issues in consequence of the operation from the fish, yields a natural pickle, and improves the flavour; whereas, if left in the fish, it becomes coagulated at the back-bone, and forms the first cause of decay. The mixture of blood and salt operated upon by the extreme heat of the weather during the summer fisheries produces a fermenta-

tion

tion which nearly parboils the herrings, and removes the coarse and raw flavour so often complained of. The gypping is likewise often performed on shore, observing the same precautions; the only difference is, that they are seldom in that case of so good a colour. Gypped herrings are never of so fine a quality as when kept in their own original pickle; their value consists in their softness and flavour; it is this mode of curing herrings that used to be the pride of the Dutch, and this is the kind which supplied their home consumption, and were so much esteemed by all classes of people in Holland.

Difference in their value.

In order, as far as it is possible, to give a proof of the correctness of the above assertion, I shall state a fact for the information of the Society. During the last year I employed a number of Dutch fishermen, prisoners, and others, with Englishmen, in gypping and curing herrings; and at one time my agent at Yarmouth was offered £4 per barrel, for all the herrings he had cured there, by a Dutch captain, in order to their being taken to Holland, while ungypped herrings were worth only 36s. per barrel. The herrings now under the consideration of your Society are part of the quantity for which this offer was made.

Should the Society, after due consideration, think proper to adjudge me their gold medal, it will afford me much satisfaction, and convince me, that my exertions have, in some degree, been beneficial to the community.

I am, Sir,

Your most obedient servant,

FRANCIS FORTUNE.

No. 9, Lower Thames Street,
Feb. the 26th, 1811.

IX.

IX.

Method of Curing Herrings: by Mr. SLEAVIN*.

WHEN the herrings are taken and alive, break their gills with your finger and thumb completely from the back-bone, which will in course cause the fish to bleed: then throw them into the liquor prepared as follows: viz. to three quarts of salt water, put five pounds of common salt, and two pounds of bay salt, and when dissolved, let the whole be boiled. One peck of common salt, and half a peck of bay salt, put between the different layers of herrings, will be sufficient for one barrel. Let the herrings remain in this liquor for three weeks, they must then be taken out and gypped, and a fresh liquor made with one gallon of salt water, the gypping of the fish, one peck of common salt, and a quarter of a peck of bay salt, and when dissolved, some of the spare fish mustbe put in to it to make the liquor rich, and the whole be boiled for an hour, but so slow as that it may not burn; then let it cool and strain it off. The fish must be repacked in clean barrels, the last mentioned liquor put to them, and be careful that the fish be covered and kept close.

Another mode of curing herrings.

P. SLEAVIN.

No. 7, Little Brook Street, Hampstead Road, April the 6th, 1811.

X.

Letter on the Structure of the Water Lily, in answer to a Correspondent. By Mrs. AGNES IBBETSON.

SIR,

THE gentleman, who did me the honour to notice my letter on water plants, in your last number, p. 22, is per-

Structure of the water lily.

* Abstracted from the Trans. of the Soc. of Arts, vol. XXIX, p. 162. Mr. Sleavin cured only eight barrels, of thirty-two gallons each, of herrings caught off the Isle of Man. Nothing is said of their quality, except Mr. Sleavin's assertion, that he has no doubt they are equal to the Dutch, or better. The silver medal of the Society was voted to him.

fectly

fectly right: the mistake of my amenuansis, who inserted "washed off" instead of "rubbed off," has caused an apparent confusion in the description. No water can enter the air vessels, except when the adjoining parts are much torn.

Motion of the hairs.

The motion of the hair in the air cylinder is caused by a pin, which, entering the widest end of the hair, runs through the side of the air vessel into the next water vessel. The water rising contracts the spiral, pushes the pin, and the hair, which remained before parallel to the side of the vessel, now starts up horizontally; and, as the whole circle of hairs rises, each in the same manner and at the same time, meeting with their points, if any insect has placed itself in the way, it will be crushed or run through (as I have often seen it) by the sudden motion of the hairs. No insect certainly can get into the air cylinders, but by the dilapidations of some of the adjoining vessels; but this must often happen, as these plants are the food of many of the diminutive slug kind; and I doubt whether the cicaria lemnæ (which is the only species I have found on the hairs) do not also feed on it.

The plant the food of many small slugs,

and perhaps of the cicaria lemnæ.

I am, Sir,
Your obliged servant,
AGNES IBBETSON.

XI.

On the Irritability of the Sowthistle, and other Plants, with farther observations on the Irritability of Vegetables: by D. J. CARRADORI*.

Irritability of plants.

THE lettuce is not the only plant that possesses a striking degree of irritability during the period of flowering, the prickly sowthistle *(sonchus asper)* has this faculty at the same season. In fact it transmits and gives out a milky fluid, like the lettuce, when it is irritated, or punc-

Shown in the sowthistle,

* Abridged from the Journ. de Phys. vol. LXVII, p. 405.

† Experiments on the Irritability of the Lettuce, Mem. of the Italian Society of Sciences, vol. XII.

tured

tured, at that time; though not so quickly as the lettuce, or with the same facility and force. It requires a stronger irritation, or a more powerful and complex stimulus, to excite the flow of the milky liquid in this plant; and does not obey the slightest touch like the lettuce, which, as soon as it is touched, however gently, throws out a portion of its proper or milky juice.

This exudation is never performed with the same force as in the lettuce, from which it is sometimes spirted out into the air to some distance; but simply flows out, however powerful the irritation. Neither is it obtainable from the leaves that embrace the stalk, as in the lettuce, but from the calices alone, and chiefly from the circumferences of the little leaves that compose these. and more strikingly in the lettuce.

The sowthistle, like the lettuce, does not lose this faculty when immersed in water; and the plant, if pulled out of the ground, or a single branch of it, will retain it some time. Not destroyed by immersion in water.

I have not had time to extend my observations to the other species of the lettuce and sowthistle, to find whether, either while flowering or at any other time, they gave signs of a sensible degree of irritability in any part by a similar exudation, though this is probable. I have found it in the bark of the fruit when green, or in the pericarps of these plants. Found in the pericarp.

I could not obtain the customary exudation from the leaves, the stalks, the parts that support the organs of fructification, or any other part, in whatever way I irritated them, except from the green capsules containing the seeds: and the irritation was always produced by a needle, or rubbing; never by any method capable of tearing or injuring the surface of the capsules*. Other parts void of it. How excited.

* There are motions in plants not owing to irritability, but the simple effects of the elasticity of certain parts, as in the great mullein (*verbascum sinuatum*). If a shock, or commotion, be given to the stalk of this plant, the flowers will fall off; not immediately, nor all at once, but a little while after, and successively. This is owing to the elasticity of the calices, which are kept in a state of forced distension to hold the monopetalous flower, which is not attached to them; and as the shock causes them to contract, by calling into action their natural elasticity, this contraction gradually expels the flower. The mullein made to shed its flowers.

I find

Effect of saline impregnations on the irritability: I put some small lettuce plants, while in flower, with their roots, to vegetate in water, to which was added in some of the vessels a small portion of muriate of soda, in others of nitrate of potash, and kept them thus in the open air some days. If any portion of these salts were absorbed from the water, they did not appear to increase the irritability of the plants, for the exudation diminished in quantity.

of acids: I mixed acids also in water, in such proportion as to be barely sensible to the taste, and particularly nitric and oximuriatic acid in various proportions, and then placed several small plants of lettuce in full flower in the vessels, with similar effect. If a larger quantity of acid were added to the water, it appeared to injure the plants, and their irritability likewise decreased more quickly.

and of oximuriatic I applied this kind of stimulus to the surface of plants, to see if it would act externally. On immersing a branch of lettuce in a jar filled with oximuriatic acid gas, and taking it out in a few seconds, it exhibited the usual exudation when stimulated: but when it was kept longer in the gas, it was evidently injured; and its *irritability* greatly decreased. Nitrous and sulphureous vapours were

and other gasses. still more injurious.

It appears then that these stimuli, which are so much vaunted as inceasing the irritability of animals, are not appropriate to the irritability of vegetables*.

* Those facts, that are admitted as proofs of the stimulant action of certain substances or principles in the vegetable economy, do not appear to me decisive.

Oxigen does not act as a stimulus to vegetables. It is generally supposed, that oxigen is a powerful stimulus to vegetables, because it has been observed to accelerate the germination of seeds; this effect being ascribed to its stimulating their vascular system, and rendering their circulation more active. But as it appears from the observations of Mr. de Saussure the younger, that the oxigen entering into germination is neither absorbed nor assimilated in this process, but employed in forming carbonic acid, I conceive it does not act as a stimulus, but merely serves to carry off from the germinating seed the carbon; an element which, as is shown in some of my observations respecting the action of light on germinating seeds, inserted in the *Opuscoli scelti* of Milan, seems injurious to the developement of the embryo; and of which nature seems at that time disposed to free herself, as noxious or superfluous. This appears to be the reason why oxigen accelerates germination.

I immersed

I immersed lettuce plants in some stagnant water, from which fetid exhalations rose, and kept them in it twenty-four hours. Having taken them out, and repeatedly examined the state of their irritability by stimuli, I found they had lost it entirely. The vessels containing the proper juice of the plants were so deprived of irritability, that they did not emit their fluid, even though wounds were made in the plants for the purpose. It seems therefore, that putrid exhalations, or putrid matter combined with water, deprive these plants, as well as animals, of their irritablity.

Lettuce immersed in stagnant water:

Having taken a lettuce plant, when Reaumur's thermometer was at 25° in the shade [88·25° F.], I immersed it in water at 50° [144·5° F.]; a degree of heat I had found not to injure the organic texture of vegetables. In this hot fluid there was a spontaneous exudation from the plant; and at the slightest touch it gave out its juice more freely than in air at the same temperature. I then immersed it for a moment in water at 4° [41° F.]; and, after waiting for a few seconds, that it might have become sensible of the effect, I irritated it afresh; when I found it required a much stronger irritation.

exposed to a high and low temperature.

The irritability of vegetables appears to be increased by heat, and diminished by cold. Vegetables in fact slacken in the exercise of their functions, if they do not suspend it entirely, during the cold weather; and the spring, which brings with it warmth, restores to the vegetable economy its accustomed energy. By this it appears their sleeping irritability is awakened, and their life revived; so that the state, in which vegetables pass the winter, may be compared with the torpor, or lethargy, that many animals undergo during that season. Cold benumbs animals, because as is well known, it deadens their irritability; and this it does by its direct action on the muscular fibre, which is the seat of irritability, independent of sensation and circulation, as Spallanzani has shown.

Irritability of vegetables increased by heat and diminished by cold.

Into a deep well, where the thermometer was at 12° [59° F.], I put a plant of lettuce in full flower; taking it from a kitchen garden, where the thermometer stood at 26° [90·5° F.] in the shade; and kept it there some hours, with its root only

A moderate temperature does not affect it.

in

in the water. Having taken it out, I found by repeated trials, that the exudation followed irritation, as it appeared to me, nearly as before; so that I could not find any perceptible difference in its irritability after it had been exposed to this cool temperature.

Thus the irritability of vegetables does not appear to suffer by a sudden transition from a high to a moderate temperature, or to be diminished in proportion to it: though the preceding experiment shows, that, when their irritability has been heightened by a very hot atmosphere, and they are placed for an instant in a cold one, it is perceptibly diminished.

Light has no effect on the irritability.

Light is well known to act as a stimulus on plants: but I did not find greater marks of irritability in the lettuce or sowthistle when surrounded by sunshine, than when in the shade.

I tried the effects of the solar light concentrated by a lens on these two plants; but it did not produce any irritation, so as to cause the exudation of the usual fluid, though it scorched them, when sufficiently intense.

Life and irritability extinguished together.

I pulled up some whole plants of lettuce and sowthistle, and also stripped off some branches, and left them to wither on a table in my room in summer. About ten hours after I irritated them where the effect would be most visible, and obtained some slight marks of irritation. I then placed the stalks of one or two of these plants in water; and after some time I found they recovered from their apparent death, and began to vegetate afresh. A little time after I attempted to irritate some others, that were still more withered; but they exhibited no exudation. I then put them in water like the former, but they never recovered. Thus in plants life and irritability appear to become extinct together.

Irritability strongest in the morning.

I tried to irritate plants of lettuce and sowthistle, growing in the same ground, at various hours of the day and night; and I found their exudation most energetic in the morning, when the sun had risen, and their flowers were fully expanded.

This irritability probably common to all plants,

The property, that lettuce, sowthistle, and spurge have, of giving out a milky fluid, or their peculiar juice, when any of their more succulent parts are irritated, appears to me, to render the existence of irritability in plants unquestionable.

It

It is true, that we perceive this irritability only at a certain age, and not in all plants that have a peculiar juice. But are we to presume, that, if this property do not manifest itself at every age, and in every plant, but only when it is extremely exalted, and in those plants that are perhaps most endued with it, other plants are destitute of it? On the contrary we may reasonably infer, that those vessels, which exhibit a great deal of irritability at a certain period, and in certain plants, possess at other times, and in other plants, a sufficient quantity for the circulation of the fluids, though no excess of it to be rendered sensible.

But if it be reasonable to suppose, that the vessels containing the peculiar juice are endued with this irritability, and that it is by this property the juice is compelled to circulate in them; who will venture to assert, that the vessels of other systems are destitute of it, and that the circulation of their respective fluids arises from a different cause, or is occasioned by some other power? and to all their vessels.

XII.

Chemical Examination of some Vegetable Substances; by Mr. VAUQUELIN*.

SECT, I. *Chemical Examination of a vegetable excrescence from Madagascar, sent to the Isle of France by Mr. Chapelher, and thence to Europe by Mr. Jannet.*

THIS substance is as white as a cake of starch; it is perforated in all directions by an immense quantity of holes formed by little insects; it has neither smell nor taste; it diffuses in burning the smell of burned bread, inclining a little to that of touchwood. Vegetable excrescence from Madagascar described.

1. Treated with a very large quantity of nitric acid, it furnished a little oxalic acid, but no muric; consequently it contains no gum.

2. Water has no action on it: but if it remains a long time

* Ann. de Chim. vol. lxxii, p. 297.

time in this liquid, at a temperature sufficiently high, a portion of the animal matter, which appears to be contained in it, undergoes putrefaction, and imparts to the water a fetid smell, analogous to that of cauliflowers; which appears to indicate the presence of sulphur.

The portion that remains still enjoys all its properties.

3. Acetous acid, boiled with this substance, takes from it some matter, which appears to be of an animal nature; for it is precipitated by galls, but not by alkalis. What is not dissolved by the vinegar possesses the same properties as before, or at least nearly so.

3. Ten grammes [154·45 grs] of this matter, subjected to distillation, yielded an empyreumatic oil, mixed with an acid liquor, which diffused an ammoniacal smell, when potash was mixed with it.

The coal, when burned, left 1 dec. [1·54 gr.] of yellowish ashes, containing a little phosphate of lime, some carbonate of lime, and a trace of oxide of iron.

This matter having the appearance of starch, or at least seeming to contain some, a principal object of all the experiments made with it was to discover this; but not the least trace of it could be detected.

Result. From this examination it seems to result, that the substance is a mixture of unorganized woody matter, and of vegeto-animal matter, which, having been superabundant in the vegetable, were expelled to its exterior, and there formed an excrescence.

SECT. II. *Analysis of a gum-resin, sent in the year 13 from Madagascar to the Isle of France, by Mr. Chapellier, and thence to the Museum of Natural History by Mr. Victor Jannet, in November*, 1808.

Gum-resin from Madagascar. This gum-resin is of a greenish brown colour. It burns swelling up, and emitting a thick smoke, with a smell not very pleasant; and leaves ashes containing carbonate of lime.

Analysed. Alcohol, assisted by a gentle heat, dissolves a great part of it; leaving a residuum greasy to the feel, which alcohol attacks only when boiling, and the greater part of which separates

separates immediately on cooling. The matter that thus falls down in cooling exhibits all the properties of lac. Its weight is six hundredths of the resin.

The portion of the resin, one tenth, on which the alcohol had no action, was treated with caustic potash dissolved in water. This had not much more action on it than alcohol; leaving it in the form of a brown powder, soft to the touch, and still weighing near one tenth.

This substance, insoluble both in alcohol and potash, was distilled with a gentle heat. At first it gave out a little water; and then vapours arose, which condensed into an oil, and a liquid of a taste somewhat aromatic, without being disagreeable, having a great resemblance to the product of gums.

None of the products of this distillation, mixed with quicklime or with potash, yielded the least trace of ammonia. The coal in the retort was easy of incineration, and left a decigramme of yellowish ashes, containing some lime, and a little oxide of iron.

The alcoholic solution of the resin had a brown colour and a peculiar taste. Being evaporated to dryness in a retort, the alcohol that came over contained nothing aromatic.

The resin was boiled in water, to which it communicated a slight taste. Thus purified it had a yellowish brown colour. It retains water pretty strongly, for it is difficult to dry, and remains soft a pretty long time.

Thus it appears, that the substance which we have called a gum-resin contains, in 10 grammes, Component parts.

1, Lac	0·6
2, Residuum, containing a little more lac and vegetable matter	1·0
3, Remains for the weight of the resin	8·4
	10.

This is I believe the first time, that lac has been found mixed with other resins; and this fact confirms us in the opinion, that the same vegetable may form several kinds of resins, as well as different trees produce the same resin. One plant may yield different resins, and different plants the same resin.

SECT. III. *Analysis of the root of camel's hay, andropogon schœnanthus, L., sent from the Isle of France by Mr. Jannet, in* 1808.

Root of sweet rush. This root has a yellowish colour, and in smell resembles Virginian snakeroot.

Treated with alcohol, Twenty grammes [308·91 grs] were infused in alcohol, which was renewed, till it no longer acquired any colour.

The filtered alcoholic solutions had a fine golden hue. Subjected to distillation, alcohol came over, the first portions of which had no foreign smell; but as the liquor in the retort became less spirituous, and required more heat to keep up the ebullition, the weaker spirit that came over had a perceptible smell, a little resembling that of the root.

The matter remaining in the retort became turbid, and was decanted boiling hot into a capsule. On cooling it let fall a brown oil.

The supernatant liquid had a yellow colour; and a very little taste, slightly saline, and a little aromatic. The oily sediment was thick, smooth to the touch, had an acrid, burning taste, like an essential oil, and in smell greatly resembled myrrh.

with water, The 20 gr. of the root, after being exhausted by alcohol, were boiled in water. The decoction, after being concentrated, had a yellow colour, and very little taste; it did not precipitate sulphate of iron or gelatine; it was not rendered turbid by alcohol or infusion of galls; it reddened infusion of litmus pretty strongly, but, as the liquor was in small quantity, the nature of the acid could not be ascertained: thus the alcohol had left the water scarcely any thing to dissolve.

and dilute nitric acid. After the root had been boiled in water, it was infused in diluted nitric acid. This infusion gave with ammonia a very slight precipitate, which resembled oxalate of lime; but there was too little of it, to be certain of its nature.

Incinerated. 20 gr. [308·91 grs] being incinerated left a red residuum weighing 8 dec. [12·36 grs]. This residuum dissolved in muriatic acid with a very slight effervescence. The solution had a fine yellow colour, and gave with ammonia a bulky precipitate of a deep brown colour. Treated with caustic pot-

ash,

ash, this precipitate afforded a little alumine; but the alkaline liquor did not give the least trace of phosphoric acid.

The ammoniacal liquor, from which the oxide of iron had been separated, yielded a little lime to oxalic acid. The residuum left by the caustic potash was oxide of iron.

Thus this root contains,

Substances contained in it.

1, A resinous matter of a deep brown red, with an acrid taste, and a smell exactly similar to that of myrrh. In fact we believe it is nothing but resin of myrrh.

2, A colouring matter soluble in water.

3, A free acid.

4, A calcareous salt, the species of which we could not ascertain.

5, Oxide of iron in pretty large quantity, the state of combination of which in the plant we do not know.

6, A large quantity of woody matter.

Myrrh may be formed in several plants.

The most interesting result of this analysis is the presence in the andropogon schœnanthus of a resinous matter, altogether similar to the resin of common myrrh; it differs only by being a little less solid, but if it were mixed, as in myrrh, with a certain quantity of gummy matter, I have no doubt it would resemble it perfectly. Hence we may infer, that myrrh is formed in several vegetables; for, though we are unacquainted here with the tree from which the myrrh of the shops is derived, it is probably not the andropogon schœnanthus.

SECT. IV. *Analysis of the aromatic leaves of the raventsara, agathophyllum ravensara L, sent by* Mr. THOUIN.

Leaves of raventsara treated with alcohol.

I digested 15 gr [231·68 grs] of these leaves in alcohol at 36° [0·837], to which they gave a fine green colour. I repeated the digestion, till the alcohol acquired no colour assisted by heat.

The solutions, when mixed, were of a fine green. On cooling a small quantity of flocculent matter separated, which I found to be wax.

The alcohol, freed from this matter, was distilled in a retort. The spirit that came over had a very pleasant smell and taste.

The remainder was rendered turbid by a little green vegetable resin. When this was separated by filtration, the liquor was of a fine brown yellow. On standing a small

 quantity

quantity of brown matter was deposited; after which a few drops of oil collected on the surface having the taste and smell of oil of cloves.

Yielded an oil.

The liquor, evaporated spontaneously in the open air, yielded a pretty considerable quantity more of this brown oil, and a clear liquor, as thick as a sirup, which had the taste of oil of cloves mixed with bitterness.

The leaves boiled in water.

I boiled in water the leaves exhausted by alcohol, but they only imparted to it a slight yellow colour, and the property of faintly reddening infusion of litmus, and being copiously precipitated by alcohol. This decoction was not affected by infusion of galls, solution of sulphate of iron, or gelatine.

and incinerated.

After the leaves had been drained, I incinerated them, and from the 15 gr. [231·68 grs] employed obtained 7 dec. [10·81 grs] of carbonate of lime, mixed with a little phosphate of the same earth.

As it is to be presumed, that this lime was combined with oxalic acid in the leaves, I digested 8 gr. [12·36 grs] in nitric acid diluted with a great deal of water; but the acid liquor yielded a very little precipitate when saturated with ammonia.

The oil similar to that of cloves.

The oil we obtained from the raventsara exhibited absolutely all the properties of the essential oil of cloves; its colour, smell taste, and specific gravity, which is a little greater than that of water. It differed only by being a little more consistent, which was probably owing to the leaves being old, so that the oil had been thickened, and in some sort resinified, by time.

Different plants may form the same oil.

From this analysis we may infer, that vegetables of different species are capable of forming an essential oil of the same nature.

The leaves a substitute for cloves.

These leaves might be employed for domestic purposes instead of cloves, using them only in larger quantity.

XIII.

Of the Efficacy of Plumbago against Tetters; by Dr. WIENHOLD*.

Plumbago employed as a medicine.

PLUMBAGO is a natural compound of nine parts carbon with about one of iron†, forming the carburet of iron of the

* Ann. de Chim. vol. LXXVI, p. 198

† Carbon 96, iron 4. C chemists.

chemists. No one seems to have thought of introducing it into the materia medica, unless in the polar regions, where the people not only rub themselves with it daily, but employ it against cutaneous eruptions. This fact, added to its known property of exciting animal electricity, and conducting it like metals, induced Dr. Wienhold to make trial of it: and in the General Medical Annals of Altenburg, for May, 1809, he published his observations and remarks on it; from which he affirms, that he can recommend it by experience against all tetterous eruptions; as, whether simple or complicated, they yield to its internal and external application, provided it be joined with medicines appropriate to their different complications; as iron, muriate of lime, and dulcamara, in scrofulous; aconite and guaiacum in arthritic; mercury in siphylitic; and sulphur in psoric tetters. In herpetic diseases.

In the latter, which neither sulphur alone nor black lead alone would cure, he has always been speedily successful, on giving the patient daily a drachm of graphitic ethiops, made by triturating together equal parts of sulphur and plumbago. Graphitic ethiops.

We shall not here enter into all the modes of administering this remedy, which the author has varied according to the cases; the formulæ he has given for their preparation; and his remarks on their mode of acting; which may be seen in No. 85 of the *Bibliothèque médicale*, we shall only add, that, for want of English black lead, being obliged to use that of Passau, he found, that it was less efficacious, required to be given in a larger dose, and, not being reducible to so fine a powder, did not sit so easy on the stomach. It is indeed well known, that the plumbago of Passau, though it does not contain pyrites like that of Spain, is much more loaded with foreign matter. To those who may be inclined to try this remedy however, we believe we may point out as preferable, on account of its purity and the fineness of its grain, that which is found in the valley of Lucerne, or of Pellis, in the circle of Pignerol, in the department of the Po, where it forms a vein two feet thick by three broad, according to the description given by Mr. Bonvoisin in the Mem. of the Ac. of Turin, 1805, p. 182. Plumbago differs in quality.

XIV.

XIV.

METEOROLOGICAL JOURNAL.

1812.	Wind	Pressure. Max.	Min.	Med.	Temperature. Max.	Min.	Med.	Evap.	Rain	
4th Mo.										
April 4	E	30·10	29·60	29·850	53	36	45·5	—		☽
5	S	30·18	30·10	30·140	56	35	45·5	—	4	
6	S	30·10	29·93	30·015	53	42	47·5	·25		
7	E	30·02	29·88	29·950	50	37	43·5	—	·10	
8	N E	30·15	30·02	30·085	49	25	37·0	—		
9	N E	30·15	30·01	30·080	43	33	38·0	—		
10	N E	30·01	29·98	29·995	47	36	41·5	—		
11	W	29·98	29·89	29·935	44	37	40·5	·43		●
12	N E	30.05	29·89	29·970	52	32	42·0	—		
13	N E	30·05	30·05	30·050	51	33	42.0	—		
14	N E	30·05	29·91	29·980	51	32	41·5	—		
15	N E	29·91	29·73	29·820	53	30	41·5	—		
16	N	29·73	29·64	29·685	49	27	38·0	—	—	
17	N	29·83	29·64	29·735	48	28	38·0	—	—	
18	N	30·09	29·83	29·960	51	29	40·0	·87		☾
19	Var.	30·09	30·02	30·055	50	33	41·5	—		
20	N W	30·15	30·00	30·075	58	30	44·0	—	—	
21	N E	30·15	30·07	30·110	58	37	47·5	·28		
22	N	30·01	29·97	29·990	54	30	42·0	—		
23	—	——	——	——	52	32	42·0	—		
24	W	29·94	29·86	29·900	52	36	44·0	—		
25	S W	29·86	29·56	29·710	54	39	46·5	—	—	
26	Var.	29·65	29·55	29·600	49	34	41·5	·69	·47	○
27	N E	29·64	29·59	29·615	52	44	48·0	—	6	
28	N E	29·76	29·64	29·700	51	44	47.5	—	·14	
29	N E	29·80	29·76	29·780	52	43	47.5	·27	·39	
30	Var.	30·02	29·80	29·860	55	45	50·0	—	—	
5th Mo.										
May 1	E	30·02	29·92	29·970	59	43	51·0	—	—	
2	N E	29·92	29.73	29·825	54	42	48·0	—	—	
3	Var.	29·75	29·70	29·725	56	32	44·0	·55	4	
		30·18	29·55	29·902	59	25	43·57	3·34	1·24	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES.

NOTES.

Fourth Month, 4. Cloudy a. m. Clear evening. 5. Much dew: barometer unsteady: heavy clouds through the day: a shower about sunset. 6. Much dew: gray sky, and the air nearly calm. 7. Lightly cloudy: little wind. 8. Cloudy a. m.: a shower p. m. 9. Brisk wind: cloudy. 10. Hoar frost. 11. Cloudy. 16. Slight showers. 17. Little hail. 20. a few large drops. 23, 24. Occasional slight showers of hail, &c. 25, 26. Gentle showers of rain yet not warm. 27. Misty morning: much dew: swallows appear. 28, 29. Cloudy: windy.

Fifth Month. 1, 2. Cloudy: the cuckow heard. 3. About 1 p. m. a few drops of rain, attended with the smell of electricity in the air: the wind, which in the morning had been brisk at N. E., died away, the canopy of the sky rose: the evening was calm, and dew fell.

RESULTS.

Prevailing winds N. E.

Barometer: highest observation 30·18 inches; lowest 29·55 inches; Mean of the period 29·902 inches.

Thermometer: highest observation 59°; lowest 25°; Mean of the period 43·57°.

Evaporation 3·34 inches. Rain 1·24 inches.

PLAISTOW. L. HOWARD.
Fifth Month, 24, 1812.

XV.

Experiments on Camphoric Acid; by Mr. BUCHOLZ*.

DOERFURT imagined he had shown by experiment, that the camphoric acid, described by Bouillon-Lagrange, was

Camphoric acid distinct

* Journ. de Phys. vol. LXX. p. 347. Translated from Gehlen's Journal by Mr. Vogel.

from the benzoic, was similar to benzoic acid. Bucholz has lately resumed the subject, and shown, that the camphoric is a peculiar acid. The following properties sufficiently distinguish them.

in crystallization, 1. The camphoric acid is crystallizable by slow refrigeration. The crystals, as Bouillon-Lagrange observed, greatly resemble plumose muriate of ammonia. The benzoic acid, on the contrary, under the same circumstances crystallizes in small needles, or in ribandlike laminæ.

taste, 2. The taste of camphoric acid is very sour, and leaves a bitterness behind; while that of the benzoic is sweet, saccharine, little acid, pungent, and excites coughing.

solubility in water, 3. Camphoric acid dissolves at 15° R, [65·75° F.] in 100 parts of water, and at a boiling heat in ten or eleven. Benzoic acid requires 24 parts of boiling water, and 200 at 15° [65·75° F.]

and in alcohol, 4. One part of alcohol at the common temperature dissolves 1·06 of camphoric acid; and 92 parts of boiling alcohol dissolve 146, or even more. Benzoic acid requires its own weight of boiling alcohol, and twice as much cold.

phenomena of sublimation, 5. Camphoric acid is capable of being sublimed as well as the benzoic, but the appearances are very different. In the first place it sublimes more difficultly: a great quantity is decomposed: an empyreumatic oil is produced with a smell of navew, an acid liquor, and a great deal of coal: and the sublimate has not a crystalline form. The benzoic acid sublimes in crystals, and yields no aqueous vapour, very little oil, and much less coal than the camphoric.

(Properties of the sublimed acid.) The camphoric acid when sublimed has a pungent and slightly acid taste. On account of the oil it dissolves more slowly in water. This solution reddens litmus paper.

and action on bases, 6. The camphoric acid comports itself very differently with respect to the salifiable bases.

particularly lime. The camphorate of lime exhibits a striking difference from the benzoate of lime.

A hundred parts of camphoric acid require for their perfect neutralization 36 parts of carbonate of lime; while the same quantity of benzoic acid requires $\frac{14}{10}$ parts.

The camphorate of lime crystallizes difficultly in rounded heaps

heaps; the benzoate, in shining stellar laminæ: The camphorate of lime has a slightly saline bitter taste, leaving a calcareous taste behind: the bonzoate is sweet, and a httle earthy.

The camphorate of lime exposed to heat furnishes an aromatic oil, resembling that of rosemary in smell; no crystallized substance passes over; and the camphorate does not melt.

If the benzoate of lime be treated in the same manner, crystals of benzoic acid pass over into the receiver, with an empyreumatic oil having a smell of balsam of Peru; and the benzoate remaining in the retort becomes perfectly fluid.

The camphorate of lime dissolves in five parts of water at a common temperature; while the benzoate requires twenty parts.

XVI.

Inquiry concerning the Means of Knowing the Proportions of Acid and Potash, that enter into the Composition of Sulphate of Alumine, and of Sulphate, Nitrate, and Muriate of Potash: by Mr. CURAUDAU, *Prof. of Chemistry applicable to the Arts, and Member of various Literary Societies**.

Products of a large alum manufactory at variance with the admitted proportions of the salt.

IN undertaking the present inquiry I had no intention of verifying the experiments of those celebrated chemists, who have endeavoured to ascertain the quantities of acid and base, that enter into the composition of sulphate of potash; I was merely desirous of knowing why the annual results of the alum manufactory, that I have established at Vaugirard, were very far from agreeing either with the quantity of acid, or that of potash, which different analyses indicate as contained in sulphate of potash and in alum. For instance, when, instead of 31 parts of acid, the quantity designated as entering into the composition of 100 parts of alum, 43 or 44 are required; and, instead of ten parts and half of potash, fifteen and half are required for

* Journ. de Phys. vol. LXVII, p. 5. Read to the Imperial Institute, April the 4th, 1808.

100 parts of alum; such an increase in the quantity of materials could not fail to engage my attention, and lead me to seek the cause of so great a difference between the results of analysis and those of a manufactory on a large scale. At first I suspected, that the surplus of acid and potash I employed entered into the composition of the insoluble sulphate of alumine, which, I have remarked, is sometimes formed. Indeed I was long induced to entertain this opinion, rather than suppose, that the quantity of acid and of potash entering into the composition of alumine were more considerable, than had been fixed by different analyses, on the accuracy of which I had always depended.

An insoluble sulphate of alumine sometimes formed.

Desirable to prevent this.

However, admitting the hypothesis of the constant formation of an insoluble sulphate of alumine, I could not remain indifferent to the loss of this substance: on the contrary, it was an object with me to find the means of preventing the alum from passing to this state of insolubility. Accordingly, as soon as I was certain, that all the acid and potash employed entered into the composition of the alum I manufactured, I was convinced, that my former observations had been just.

Attempt to ascertain the proportions of potash and acid in alum.

But as I was not satisfied with being merely convinced of what was in favour of my observations, it remained for me to ascertain by direct experiments, and particularly such as could easily be repeated, how much acid and potash enter into the composition of alum. I wished also to learn, whether the quantities of acid and base in the sulphate of potash were such, as are generally admitted. Lastly, that my experiments might not be suspected of the least inaccuracy, it became necessary, that I should prepare some very pure sulphate of alumine; a circumstance that enabled me to obtain this sulphate very regularly crystallized, a state in which it had not yet been known, since its concentrated solution yields only lamellar, micaceous crystals, always of an irregular figure. I have had the honour of showing crystals of this sulphate of alumine to several members of the class, particularly to Mr. Haüy, who was very desirous of adding a specimen of this sulphate to his valuable collection.

Pure sulphate of alumine crystallized.

In

In a paper, which I shall have the honour of communicating to the class, I shall make known the physical properties of this saline substance, as well as the means and conditions requisite, to promote its crystallization.

When I had at my disposal a certain quantity of this sulphate, it was easy for me to find with precision the proportions of potash and acid, that enter into all the salts with base of potash. I satisfied myself also, that this sulphate of alumine is a very powerful and certain test for ascertaining the quantity of potash contained in vegetables, either before or after incineration. On this subject I have undertaken several experiments, that will complete another inquiry, which I shall have the honour of submitting to the class.

This sulphate useful in analyses.

To return to the analysis, or rather the synthesis, that constitutes the subject of the present paper: the following are the experiments I have made, to determine the respective quantities of acid and base, that enter into the composition of alum, and of the sulphate, nitrate, and muriate of potash.

Exp. 1. In 850 gr. [13129 grs*] of solution of sulphate of alumine at 34° [sp. gr. 1·307], the temperature being 10° [50° F.], I dissolved by the assistance of heat 100 gr. of sulphate of potash. After the liquid was cooled, I obtained from it 502 gr. of very pure alum. On evaporating the mother water I obtained 18 gr. of alum; and a second evaporation and crystallization produced 4 gr. more. The remaining liquor yielding no more crystals, I mixed it with 25 gr. of a solution of sulphate of alumine similar to that above, in order to find whether I had obtained all the alum, that 100 gr. of sulphate of potash could produce. The mixture having occasioned only a slight precipitate of alum, I concluded, that the whole of the sulphate of potash had entered into the composition of the 524 gr. of alum obtained.

Quantity of alum produced with a given weight of sulphate of potash.

Exp. 2. On the supposition, that sulphate of potash contains 62 per cent of potash, I saturated 62 gr. of potash,

Quantity produced with a

* The proportions being all that is of importance, it would be superfluous to reduce the rest of the quantities: but this is given, to mark the quantity operated on. C.

purified

given weight of potash and of sulphuric acid.

purified with alcohol, with 48 gr. of sulphuric acid at 66° [sp. gr. 1·848]. I then mixed this sulphate with 850 gr. of solution of sulphate of alumine at 34°, and conducted the rest of the process as in the preceding experiment.

But what was my surprise, when, on adding together all the alum produced, I found but 408 gr., instead of 524, which the former experiment had yielded. The comparative results of these two experiments, which I varied with quantites alternately greater and less of sulphate of potash and sulphate of alumine, demonstrated to me, that the proportions of potash and acid contained in the sulphate of potash were very different from those hitherto laid down. In fact, knowing how much alum is produced by 100 gr. of sulphate of potash, and how much may be obtained with a given quantity of potash saturated afterward with acid, it was easy for me, on comparing the results of these two experiments, to ascertain by calculation the respective proportions of acid and base, that enter into the composition both of sulphate of potash and of alum.

Proportions of acid and base in sulphate of potash.

For example, since with 100 gr. of sulphate of potash I obtained 524 gr. of alum; and on the other hand, 62 gr. of potash gave but 408; I necessarily concluded, that the potash contained in 100 parts of sulphate of potash must make four fifths of its weight. But reflecting, that, on the one hand, this quantity of potash was much greater, than is generally admitted in the sulphate of potash; and, on the other, that the acid could not lose two thirds of its weight in this combination: I could not but suspect, that the potash contributed to this loss in a certain proportion, and hence sought some means of ascertaining the quantity of water it might contain. Accordingly I made a great number of experiments with this view, and with the result of which I have so much the more reason to be satisfied, as the question to be solved is very important; since even at present, while it is allowed, that potash purified by alcohol contains water, the quantity is not agreed on: for Mr. Berthollet, according to recent experiments, admits only 15 per cent, while Mr. Darcet finds twice this quantity by his*.

Water in potash.

* See Journ. vol. XXVII, p. 31.

Hence

Hence I have presumed, that to make known the result of my experiments, though undertaken with other views, might be of some advantage. In fact, finding by synthesis the quantities of potash; and of acid entering into the composition of sulphate of potash and having afterward ascertained, how much water they lose respectively in this combination; it appears to me, that the question is solved a priori. I must confess, however, that some difficulties occurred at first in ascertaining the quantity of water contained in potash; difficulties which have afforded me an opportunity of knowing, that, interesting as the experiment of Mr. Berthollet is, the treatment of potash with iron filings is not a method sufficiently precise to be conclusive. My opinion on the contrary was, as it still is, that the substances most proper for detecting the water contained in potash should not be oxidable; and that their action should be confined to the separation of the water contained in the potash. Among the experiments I made, the following appeared to me best to fulfil the conditions I had imposed on myself. Attempts to ascertain this.

Exp. 3. Twenty grammes of potash prepared in the laboratory of Mr. Vauquelin were carefully mixed with 160 of very pure silex, which must have been dry, as it was heated for two hours in a forge fire before it was used. The mixture was introduced with much caution into a glass tube about 2 cent. [7·87 lines] in diameter. This tube, one of the extremities of which was closed, weighed 72 gr., and with the mixture 252; very good weight, it is true, but this excess I ascribed to moisture attracted by the potash during the trituration. This tube I introduced into a small cylinder of sheet iron, to prevent its being fused by the direct action of the fire. This apparatus was subjected for an hour to the action of a very moderate fire. No sooner did the mixture receive the impression of the heat, than a very large quantity of water reduced to vapour was suddenly expelled, and continued to be evolved five or six minutes, after which nothing more was extricated. Experiment.

When the tube was cold, I weighed it very carefully, and found it had lost 5·5 gr. This experiment, which I repeated several times, sometimes collecting the water, constantly afforded me the same results, both with potash of my own preparing, and with that from the laboratory of Mr. Vauquelin; whence 27·5 of water in 100 of potash.

Proportions of the sulphate.

whence I conclude, that in 100 parts of potash purified by alcohol there are 27·5 of water; and, setting out with this datum, that the potash in 100 perts of sulphate of potash is 57·71, instead of 52 as assigned by Bergman.

I cannot omit remarking however, that the analysis of alums by Mr. Vauquelin* demonstrates the presence of sulphate of potash in them nearly in the same proportion, as appears from synthesis: a result showing the confidence to be placed in the analyses of that learned chemist, and leaving us to regret, that he relied on Beryman for the proportions of acid and base in the sulphate of potash.

Experiment to find the proportions of acid and base in nitrate of potash.

Exp. 4. Desirous of knowing the proportions of acid and base in nitrate of potash, I dissolved by the assistance of heat 100 gr of very dry nitrate of potash in 800 of a solution of sulphate of alumine at 34 [sp. gr. 1·307.] After the liquor was cold, I obtained 376 gr. of alum. The mother water was set to evaporate again, but as it crystallized confusedly I added 10 gr. of sulphuric acid at 66° [sp. gr. 1·848], because experience had taught me, that whenever such a solution contained an acid foreign to the alum, an excess of sulphuric acid was necessary to promote the crystallization of the alum. In fact, as soon as this mixture was made, a considerable precipitate took place, which, after being drained and dried, weighed 84 gr. Lastly to satisfy myself whether the mother water still contained alum, I added anew 100 gr. of the solution of sulphate of alumine. This addition, increasing the density of the liquid, favoured the precipitation of the small quantity of alum, which it still held in solution. When this last product was drained and dried, it amounted to 2 gr.; which, with what was obtained before, made 462 gr. of alum. As it had crystallized however in a liquid containing principles foreign to its composition, it became necessary to purify it. With this view I dissolved it, and crystallized afresh. From this process, I obtained only 452 gr. of alum, but certainly very pure.

49·76 potash, and 50·24 nitric acid.

This experiment, which I have repeated several times, and with different quantities, always gave me results confirming the former: whence I conclude, that if 100 gr. of nitrate of potash produce 452 gr. of alum, 49.76 of

* See Journal, 4to series, vol. i, p 318.

potash

potash, and 50·24 of acid, must enter into the composition of 100 parts of the nitrate.

Exp. 5. The object of this, as of the former experiment, was to ascertain, whether the base and acid in muriate of potash were in the proportions commonly admitted. For this purpose I employed the means I have just described; and, as it would be superfluous to repeat the particulars, I shall confine myself to the results. Proportions of muriate of potash.

100 gr. of dry muriate of potash, treated as in the preceding experiment, produced 607 gr. of crude alum; which, after being refined, left but 592 gr.: a result proving incontestably, that 100 parts of muriate of potash contain 65·17 of base and 34·83 of acid. This experiment, which, like the preceding, was several times repeated, always afforded me similar results. 65·17 base and 34·83 acid.

From the experiments that have been described, it follows, General conclusions.

1. That 100 parts of sulphate of potash contain 57·71 of potash and 42·29 of acid, which, from the state of concentration in which it exists in this sulphate, are equivalent to 60 parts at 66° [sp. gr. 1·848].

2. That to form 100 parts of alum requires 42·77 of sulphuric acid at 66°, instead of 30 or 31, the quantity generally admitted; 11·01 of potash; and 10·50 of alumine: a quantity equal to what was found by Vauquelin.

3. That 100 parts of very pure alum contain 19·08 of sulphate of potash, 30·92 of sulphate of alumine, and 50 of water of crystallization.

4. That 49·76 parts of potash and 50·24 of acid enter into the composition of nitrate of potash.

5. That 100 parts of muriate of potash are composed of 65·17 potash, and 34·83 muriatic acid.

6. That it is certain potash purified with alcohol contains more than a fourth of its weight of water, since, from the experiments that have been related, 27·5 per cent may be obtained from it.

7. Lastly, that by means of sulphate of alumine, with the simple base and crystallized, we may in future, in the analysis of the substances of either of the three kingdoms, detect the smallest quantity of potash contained in either: the

the method admitting of great accuracy, since the product, from which the proportion is ascertained, weighs in the proportion of 9·08 to one of dry potash.

XVII.

Analyses of Minerals; *by* MARTIN HENRY KLAPROTH, *Ph. D. &c.*

(*Continued from vol. XXXI, p. 382.*)

Triple sulphuret of lead from Cornwall.

ORE of antimony and lead from Nanslo, in Cornwall*.

Lead	39
Antimony	28·5
Copper	13·5
Sulphur	16
Iron	1
Loss	2
	100

Sulphuret of bismuth and copper.

Ore of copper and bismuth from Wittichen.

Bismuth	47·24
Copper	34·66
Sulphur	12·58
Loss	5·52
	100

Meteorolite of Agrau.

Native iron of a meteorolite from Agrau.

Native iron	96·5
Metallic nickel	3·5
	100

and Mexico.

Proust analysed a meteorolite from the province of Chaca Gualamba, in Mexico, sent by Rubin de Celis, and found in it native iron and metallic nickel.

* See Journal, vol. IX, p. 14; XX, p. 332; and XXIV, pp. 225, 251, 321; for a full account of the triple sulphuret of lead, copper, and antimony, from Cornwall, by Mr. Hatchett, Mr. Smithson, and count de Bournon.

Humboldt

A

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ARTICLE I.

On the Dissection of Flowers. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

I Have long reproached myself with not again bringing this subject before the public, as one of the most important in physiology, and that which must prove most absolutely the existence of the line of life. One of the first facts I endeavoured to show in my early letters, was, that every flower was formed by a part of the stalk appropriated to it; and that Linneus was, as usual, most absolutely right, when he advanced, that the wood formed the stamen, the inner bark the corolla, and so on to the rest of the division. The present letter should have preceded many you have received and published, as it will I hope not only explain how the mechanical work is concealed in a plant, but illustrate the fact just mentioned; enabling any person (if so inclined) to follow me in my dissections, and teach them how to seek the mechanism that belongs to each separate part. All vegetable structure is formed in one peculiar manner, that is, cylinder within cylinder; and on this curious

How to find the mechanism of plants.

curious construction most of its mechanical contrivance depends. It is strange we should annex such extreme simplicity to the vegetable form, when mineralogy is hourly presenting us with a variety of curious and difficult figures, such as to puzzle the first mathematicians to find a suitable name for their multangular solids; or a more simple derivative from which to trace their integral crystallizations. But with respect to botany the time is now come, I hope, when its mechanism will be too well known, not to show the fallacy of these ideas: for if inanimate mattter requires or can be resolved into such complex forms, how much more where motion makes mechanism constantly requisite to supersede volition; and make amends for every assistance this would bestow?

This mechanism an important study.

Each day's work in dissection more and more proves to me, that the mechanism of botany is an important science; which would develope to us, if known, the most wonderful proceedings in nature; and give us more exact notions of the sort of existence of plants (independant of volition and wholly governed by mechanical powers) than we now possess, and that the simplicity we talk of so much is merely that found in all nature; "the labour of the means never surpassing what the necessity of the end absolutely requires": of which however we are not always proper judges; for so various is the motion, so complicated the effect, to be produced, that it is impossible to dissect a single plant, and not observe some mechanical wonder, that makes one feel how little is understood of the purposes, for which it is intended; and most ardently long to attain that knowledge which to gain is now become the labour of my life. But it is in the whole general system of physiology, that that beautiful simplicity is observable, not in the mechanical part. There indeed it is unequalled: and I hope, when I come to review the whole of the present work, from the immense number of drawings I now possess, I shall prove it most exquisite.

All vegetables formed cylinder within cylinder.

By means of this curious construction of cylinder within cylinder, formed of each different sort of matter; the vessels belonging to each circle; and the juices appropriated to each vessel, can never in the smallest degree interfere with each

each other. Suppose seven or eight glass cylinders placed within one another, and having ribs of the same matter, which convey the juices of each to their appointed place; would it not be most easily understood, that the liquid thus carried, and the mechanism thus enclosed, can in no manner disturb those in the adjoining circles, though certainly increasing the size of the whole? This is exactly the case in the vegetable structure: as for example; when the mechanism is to be sought that governs the leaf; draw off the rind of the plant, and in the next matter, (that is on the bark) will be found the whole of what forms and regulates the motion of the leaves. Great care must be taken however not to carry off the balls with the rind; but, if properly stripped, the whole management of the spiral wire will then be discovered in regular order, with the balls on which it is wound, and the knots by which it is fastened.

To find the mechanism of a flower.

If the mechanism of the flower is desired, the rind is first drawn down, and displays the mechanism of the calyx, with the partial skin that leads up the vessels to its edge, and generally lines it. When this is thoroughly examined, it must be taken off with the greatest care; and it will display a green matter of a thicker kind, which is the skin on which the vessels of the corolla repose; this regular cylinder reaches up to the claw of each petal, and gives to it the vessels that are to meander through it, and the juices that inflate them. This, when properly viewed, must be cut off with a lancet, and a yellow and also a thin white skin will appear next, which hold the vessels of the stamen between them, and convey them either to the filaments or the corolla; in which they perform the rest of their journey, as in the primula; or in a skin that forms an additional cuticle to the pistil, as in the malva; or in a cylinder that stands up round the female, as in many flowers. But let it pass where it will, it always has a skin appropriated to the stamen alone; till it reaches the part where the corolla branches off; and afterward it has no connexion with the juices of the petals, though lying on them; or with the pistil, though enclosing it; as I shall now show by the dissection of a flower: I chose the peach out of several hundreds drawn in the same manner, because it is now in

season, and I could review my sketch; but all flowers in this respect are the same.

How to dissect the flower.

I shall first dissect the flower by removing skin from skin as the easiest method of making it thoroughly comprehended. I shall in the second place cut the flower down the middle; halving the pistil; by which means the interior, with the vessels which run up to each part, must be *exactly displayed*: and lastly I shall give you a vegetable cutting of the flower, just where the vessels *divide* under it, and run, up the bark to form the calyx, the inner bark to form the corolla, the wood to form the stamen, and the line of life to complete the pistil: and this will, I hope, make the description so plain, that I shall not again be obliged to return to the subject. Plate IV, fig. 1, is the bud of the peach. I first remove the scales A A, which generally go on shooting as long as the severe weather continues. I then with great ease remove the calyx. It is seldom possible to get it off whole, as it must be removed without displacing the corolla; which is difficult to do: but custom soon teaches the way. The calyx, when taken off, is seen at fig. 2: BB are the reservoirs of a glutinous liquid, resembling the juices of the bark, which appear, by varnishing the exterior, to defend it from the attack of vermin; which from its delicacy would otherwise cause it to become a complete victim. This part has no connexion with the nectary, which I shall not attempt to point out in this letter, but keep that part for a separate paper, which it well deserves. CC are the vessels which run down till branching off to the bark. Fig. 3 is next taken off. It is a green skin belonging to the inner bark; which is fastened to the corolla, and conveys the regular vessels DD from the inner bark of the stem to each separate petal. Within these vessels (as I have before observed) are the juices of the inner bark, and the spiral wires which are thus carried up to perform their office of opening and shutting the flower. Next to this is fig. 4, which is a very thin skin of white; and a very thick one of yellow matter between which are concealed the vessels which convey the juices that form the pollen, and carry it first to the filaments, and thence to the anthers. Thus they are most plainly three separate cylinders, capable of being divided, and placed together again;

Explanation of the plate.

fig. 4.

fig. 4 laid within fig. 3, and 4 and 3 within fig. 2: the line of life, and pistil alone being taken out, which are seen at fig. 5. When these are replaced the flower is perfect, and has been regularly dissected, as I promised, skin from skin; and between each cylinder the mechanism is concealed, that belongs to each division.

All flowers dissected nearly the same.

Of the many hundreds (I might say thousands) of flowers dissected in this manner, of every class and order, I never yet found one that did not admit of this arrangement. The gynandria tribe is exactly the same with respect to its cylinders, which are always to be taken off in progressive order; and let the stamens appear where they will in the flowers, their vessels always pass up in this manner, whether afterward bound to the corolla, the calyx, or the pistil.

Perpendicular section of the flower through the middle.

I shall now show the flower when divided into two halves, and cut perpendicularly down the middle. I have magnified this much, in order to show how completely the several parts are appropriated; and how separate the line of life and pistil are from every other division, till they join the stalk. At fig. 6 are three buds thus cut, without their corollas or calyxes, but having their own peculiar cylinders, which reach up to them. All within the points and the letter E is the pistil belonging to each flower, with the line of life running up to each pistil; which in the stem bounds the pith, till it stops, and then runs up to form the female. It may be seen dividing the seed at F, and halving the corculum. G is the interior of the flower. H I K are the three cylinders of the stamen, corolla, and calyx; (at least the skins to which those parts are fastened, and which conveys the vessels or mechanism up to them;) and L is the rind. I have not properly proportioned the thickness of the cylinders; as I feared they would not be seen; nature requires so thin a skin, to which it will fasten and adapt such powerful mechanism, and such a quantity of vessels, that it requires a long practice in dissection before we can give credit to our sight in this respect. I have shown several buds starting from the line of life, at T T T; and at *u* will be seen how the vessels arrange themselves, to enter the different parts of the stalk to which they belong. Fig. 7 is now my last dissection, it is a horizontal cutting of the part taken from fig. 6 at M,

to

to show in a still plainer manner how each cylinder runs up to form its appropriate part; in the flower O is the bark separated to form the calyx; P the inner bark to form the carolla; Q the wood to convey the nourishment to the stamen; and the whole interior between tne points, belonging to the pistil and seeds at R; the line of life bounding the part as well as leading up the middle. The stamens are perhaps better marked where there are fewer, I have given two very good dissections of this sort of cutting of the bottom of a flower in my third letter (see Journal, vol. XXIII Pl. IX, p. 350.) I have said, that it signifies little whether the stamen, when once past the cylinder, proceeds up the calyx, corolla, or pistil, since it has equally its peculiar vessels. Sometimes the stamens are on one side of a flower only; and then the cylinder, instead of passing in equal thickness all round; is found large only on one side; this is the case in the cutting of the violet, but it is then even more distinguishable. Often in dissecting, you find the stem suddenly enlarged; on cutting it through the middle, the pith is found still of the same size, the line of life at the same distance, and the wood not altered; but the part between the rind and bark extremely increases. When this is the case, you may be sure that it is the mechanism belonging to the leaves, or that it is a stem that turns on a ball, as in the arenarias, stellarias, and galiums; but it generally denotes the mechanism of the leaves, which is seen if the rind is drawn down. If the size appears enlarged in the wood, it is always the buds which cause it, and they will be found in numbers starting from the line of life: (as seen at T fig. 6, or at T fig. 8.)

Laburnum dissected.

I shall only add to this letter a branch of laburnum, Plate V, fig. 1; with a section greatly magnified, at fig. 2; in which are shown the flowers just shooting at V; and the line of life passing up as usual to form the pistil. As all the other parts are extremely small in proportion, they are not much marked in this sketch, which, however, very evidently shows the new wood, which is always generated for the use of the flower buds within the boundaries of the line of life at SS, and the little line of old wood which runs next the line W. It is impossible, not to see how exactly each bud shoots from the line of life, and how wholly separate the bark *y* is from the rest,

rest, and how entirely the new wood or albumen *x* divides it from the wood; it is only to recollect that these are each of them regular cylinders of different degrees of thickness, having their vessels closely applied on each part, as well as the mechanism appertaining to them, adhesive to every circle; and it will be easily understood how such delicate and complicated machinery can be each in its separate cylinder, without interfering or disturbing each other.

Uniformity of nature.

I explained in my last letter, that it was by this means, and by a perfect knowledge of the dissection of plants, that I was able to trace the male and female in the cryptogamian class. I have by the same analogy proved which are the most important lines and vessels to the vegetable kingdom. I did not set up to form a system; it is dissection alone that has created it for me; I trusted to the strict conformity of nature, and never attempted to make one part agree with the other, but drew the sketch exactly as nature presented it to me. Yet on looking back, I find they all agree; the same conformity is maintained throughout. Is not this the most convincing proof of the exactness and truth of every part? In my next I hope to give a view of the manner in which the buds shoot in annuals from the stalk; as it is really so curious and beautiful, it is well worth a letter to itself. I have also promised one on parasite plants, a very amusing and also important subject: matter presses on me so much at this time of the year, I cannot draw quick enough to keep pace with it.

I am, Sir,

Your obliged servant,

AGNES IBBETSON.

I shall add a few lines to mark the constant use of the screw in all plants, it serves a double purpose: first, that of covering and concealing the buds; and next, the easily dividing each circle to let them out when ripe. If such a specimen as fig. 9 is taken, and the rind and bark stripped off, it will appear as at fig. 10; with the wood separating into threads to let the buds pass and to make a hollow way for them. It appears to me, that there requires no other proof than

than this to show, that the buds proceed from the interior, and therefore from the line of life, for nothing can be more different than the appearance of this specimen, and of the wood in the stem, which is perfectly straight, and without any openings, while these appear in such quantities in fig. 3. Almost every tree in the spring shoots its buds in the same way, but few in such numbers as the laburnum.

II.

Remarks on the Perforations made in Paper by Electrical Batteries. In a letter from Mr. John Gough.

To Mr. NICHOLSON.

Sir,

An experiment in favour of two electric fluids stated.

MANY philosophers are of opinion, that the phenomena of electricity and galvanism are caused by the cooperation of two distinct kinds of subtile matter; which they denominate the positive and the negative electric fluids. Amongst many other facts and argument in favour of this hypothesis, the following experiments formerly appeared to me as amounting to a proof; because it seemed to be little short of a mechanical demonstration. If a quire of writing paper be placed betwixt the points of two metal rods, which are in contact with the opposite sides of it, and the charge of an electrical battery be transmitted through the wires, the bundle of paper will be perforated in the direction of a right line joining their points; and each orifice of the perforation will be surrounded externally by a bur, or prominent rim. The peculiarity of this experiment consists in the two rings or elevated borders, which are driven outward in opposite directions by the force of the discharge; and their presence is supposed to prove the existence of two opposite currents; which strike the parallel sides of the quire at the same instant, and meet in the middle of the paper, after perforating the sheets in contrary directions.

This experiment called in question and defended.

The preceding experiment happened to be the subject of conversation in a company, where I had the pleasure of meeting Mr. Webster; who lately gave a course of lectures at Kendal

Kendal in his progress northward to Edinburgh and Glasgow. This gentleman observed, that he had reason to suspect the accuracy of the foregoing statement; for, when he undertook to perforate a slip of card paper by an electrical discharge, he invariably found but one bur, and this appeared on the side of the card, which was connected with the negative surface of the battery. In consequence of this remark, the gentleman was asked, if the appearance was the same when the discharge was made from the negative to the positive side of the battery, as well as when it passed in the opposite direction, namely from the positive to the negative side.

To this question Mr. Webster replied ingenuously, that he had always made the experiments in the latter manner; and my predilection for the idea of a double current induced me to obviate, or at least to weaken the objection, by remarking, that, the positive current being put in motion before the negative fluid, it acquired a preponderance, which enabled it to drive the paper in the direction of its own course, and consequently to raise a single bur, on the side of the card that was connected with the negative surface of the battery. I moreover observed in addition to the last remark, that, if the preceding reply to Mr. Webster's objection had truth for its foundation, the place of the bur might be removed to the contrary side of the card by inverting the experiment, so as to give a preponderance to the negative current; which would then drive the paper before it, and form an elevated rim on that face of the slip, which was connected with the positive surface of the battery.

The method proposed for repeating the experiment.

The want of facts, which is apparent in this discussion, determined me to repeat the experiment with the variations and under the conditions, that had been prescribed by myself. For this purpose I procured several slips of card paper; that were cut accurately into the shape of right angled parallelograms; and all of them had both their faces divided diagonally, each by two diameters intersecting in the centre of the plane. Pieces of tinfoil were then reduced to the figure and size of the triangles, which had the shorter sides of the parallelograms for their bases. One triangle in each side of a slip, was covered in the next place by one of these metallic coatings; the pieces of tinfoil being so disposed as to

to make their bases coincide with the oposite ends of the card, while their points fell upon the centre of the surface to which they were pasted. This arrangement evidently formed an intercepted conductor; which obliged the electric charge to pass through the card paper in a right line perpendicular to its oposite faces. Perhaps I may be blamed for giving a circumstantial description of a very simple contrivance; but minuteness always appears to me absolutely necessary in relating the manner of conducting an experiment.

General result not favourable to a double current.

I made the discharge from the positive to the negative side of the battery in my first trial; in consequence of which two burs were raised at the centre of the card; namely, a small one on the face connected with the positive coating, which seems to have escaped Mr. Webster's notice, and a second on the opposite side of the paper, to which he directed his attention exclusively. This perforation bore a strict resemblance to the holes that a punch makes in a plate of metal, or other ductile substance; for I found upon trial, when an instrument of this description was driven forcibly through a card placed on a piece of soft wood, or through a plate of lead fixed by nails over a hole, the perforation made by it was furnished with two burs, like those produced by the stroke of a battery. The prominent ring surrrounding the upper orifice, where the operation of the punch began, was small; but the rim on the opposite or under surface of the card or lead was comparatively large. The reason of this difference is too manifest to require an explanation; but the strict analogy observable in the two experiments with the battery and punch led me to attribute the perforation in the former case to the action of the positive current alone. When the experiment was inverted, and the discharge made from the negative to the positive coating of the jars, no alteration was produced; for the minute bur still kept its place upon that side of the card which was connected with the positive surface of the battery; and the large bur was formed on the opposite side of the paper.

The existence of a negative fluid is not demonstrated by

This result shows the futility of my remarks on Mr. Webster's objection to a double current; for, if the positive current produced the perforation in my first experiment

periment, the same cause undoubtedly produced the same effect in the second case. This conclusion leads to another of still greater importance; for, if the perforations in question are invariably made by the positive current, the experiment under consideration affords no mechanical evidence, demonstrating the existence of a negative fluid. It will not be improper or superfluous to conclude the present letter by observing, that I made similar trials with several slips of writing paper, which were pasted together by their ends and coated with tin-foil like the cards. The result in this case was always the same; for the less bur was on the side of the bundle which was connected with the positive surface of the battery; and when the slips of paper were separated, the larger bur of each piece pointed to that face of the bundle which communicated with the negative coating of the jars.

the experiment.

MIDDLESHAW, JOHN GOUGH.
May 15th, 1812.

III.

On some Preparations of Gold lately employed medicinally: by A. S. DUPORTAL, *M. D. &c., and* TH. PELLETIER, *Apothecary**.

AFTER having enjoyed some reputation as a medicine, gold had ceased to be administered to the patient, and taken an opposite direction. Lately, however, Dr Chrestien of Montpellier, a physician of great reputation and successful practice†, has revived its use. He has employed it in siphylitic and lymphatic affections, and chiefly in Clark's mode‡. The preparations he uses are metallic gold in a state

The use of gold as a medicine lately revived.

* Abridged from Ann. de Chim. vol. LXXVIII, p. 38.

† The gentleman through whose means Dr. Godden Jones became acquainted with the virtues of d'Husson's *eau medicinale* in the gout. C.

‡ From a passage in the sequel I imagine Clark is put for Clare; and that it means by rubbing on the inside of the cheek, or on the gums C.

state of minute division, oxide of gold precipitated by potash, the oxide precipitated by tin, and the triple muriate of gold and soda. These he considers as superior to mercurials. Some experiments by Mr. Vauquelin on the preparations of gold thus introduced into notice, have already been given*, and we shall now present our readers with some remarks on the subject by the gentlemen above mentioned, one of whom enjoyed the advantage of a personal acquaintance with Dr. Chrestien, at Montpellier.

Gold in powder. How prepared by Dr. Chrestien.

The first preparation of gold employed by this physician was the metal in a state of minute division. To obtain this, he formed an amalgam, by triturating leaf gold with seven times its weight of mercury in a marble mortar with a glass pestle, and then expelling the mercury by means of a powerful lens in the height of summer, or dissolving it out by pure nitric acid.

Another mode recommended.

The present writers recommend rather to precipitate a solution of muriate of gold by a solution of sulphate of iron at a minimum, filtering, and washing the precipitate with water acidulated by muriatic acid, in order to dissolve out the oxide of iron mingled with the precipitated gold. When the gold is thoroughly dried, it is in the state of a deep brown powder, though in the metallic state; all metals losing their brilliancy by being minutely divided.

Solution of gold

To prepare the oxide of gold precipitated by potash, they direct one part of nitric acid at 40° [sp. gr. 1·396] to be mixed with four of muriatic acid at 12° [1·089]; and cupelled gold to be heated with eight times its weight of this menstruum in a matrass with a long, narrow neck, till it boils gently. When no more gold will dissolve at this temperature, the solution is to be poured off, and evaporated to dryness in another matrass by a gentle fire. The residuum of this evaporation is to be dissolved in distilled water, and filtered.

precipitated by potash.

The filtered solution is to be treated with potash, to separate from it the oxide of gold: but in this there are great difficulties, and the whole cannot be thrown down, without part of it being reduced to the metallic state.

* Journal, vol. XXX, p. 248.

The

The cause of this is not known; but the authors ascribe it, 1, to the formation of a soluble triple muriate, which takes place when the potash is poured into the solution of muriate of gold: 2, to the excess of acid always present in this muriate: 3, to the more or less caustic state of the alkali employed: 4, to the greater or less quantity of this substance added to the muriate of gold.

Oxide of gold first thrown down:

When a solution of caustic potash is poured into a saturated solution of gold by muriatic acid, a yellow precipitate is formed*, which, when collected on a filter, does not amount to more than 40 grs of oxide from 72 grs of the metal in the solution. The remaining liquid is of a very deep colour, and contains a triple muriate of gold and potash. A fresh quantity of the caustic alkali will cause no farther precipitation, unless the liquid be kept several hours in a gentle heat: but in this case a new precipitate will fall down, extremely bulky, and of a deeper colour than the former, and apparently at a different degree of oxidation. Several weeks are necessary to complete the precipitation; and even at last a certain portion of gold will remain, which must be thrown down by a slip of tin, if we would lose nothing.

a triple muriate remains in solution: from which more alkali and heat throw down gold apparently in a different state of oxidation.

Superfluous acidity of the solution to be avoided.

If the solution of gold be very acid, there will be scarcely any perceptible precipitation: and this might be expected, as the alkali finds a sufficient quantity of free acid, to form muriate of potash enough for the production of the triple salt. Indeed no precipitation at all ought to take place, when the solution is extremely acid: but here experience does not entirely agree with therory, for a very small quantity of oxide of gold is always produced.

Causticity of the potash important.

The causticity of the potash is of great importance; for, if the neutral carbonate be employed, no change will take place without the assistance of heat. This, expelling a considerable portion of carbonic acid gas, will alter the colour of the solution from yellow to greenish. If it be then filtered, traces of the purple oxide of gold will be found; and it will effervesce with acids, having its fine golden colour restored. A few drops added to a glass of water will not colour it; but, if the water be acidulated,

Action of the carbonate.

* It is necessary to employ heat.

the

Crystals produced.

the colour will instantly appear. The same solution yields by evaporation white, transparent, alkaline crystals, interspersed with black spots. These crystals dissolve in water without colouring it; and on filtering the solution it passes through transparent, leaving a little gold on the filter. The addition of any acid however causes its colour to reappear.

Their nature.

What is the chemical nature of the crystals obtained? Though this was not minutely ascertained for want of time, it appears certain, that they were composed of carbonic and muriatic acid, potash, and gold: but whether constituting a quadruple salt, a trisule, or two salts, one the triple muriate of gold and potash, the other subcarbonate of potash, the authors cannot say; nor could they form any judgment from the figure of the salt.

Carbonate of potash separates copper from gold.

It may not be amiss to observe, that, in an impure nitromuriatic solution of gold, saturated carbonate of potash will precipitate the copper, without throwing down the gold, if no heat be employed.

Too much alkali not to be employed.

As too large a quantity of alkali, added to a solution of muriate of gold, will cause a portion of the precipitated oxide to be redissolved; it is necessary, to add the alkali cautiously, boil the solution at every addition of alkali, and separate the precipitate by filtration, whenever a sensible quantity appears.

The oxide to be washed but lightly.

The precipitate must be washed but slightly, it being partly soluble in water, as Mr. Vauquelin remarked; and it must be dried in the shade and in a cool place, otherwise it will be a mixture of oxide and metallic gold.

Test of its purity.

It may be known whether the oxide be pure, by treating it with muriatic acid, which in this case will dissolve it completely; but, if it be mixed with metallic gold, part will remain undissolved.

Oxide precipitated by tin.

The oxide of gold precipitated by tin, which Dr. Chrestien also recommends, may be obtained either with metallic tin, or with its solution.

Precipitate with metallic tin.

For the first, slips of tin well cleaned are to be put into an aqueous solution of muriate of gold. These will soon be covered with a layer of pulverulent matter, of a colour more or less deep; which will be renewed several times, after being removed

removed. When this ceases to be reproduced, the liquor is to be filtered, and the precipitate washed in distilled water, dried in the shade, and powdered. This is the purple powder of Cassius.

Precipitate with solution of tin.

If the oxide of gold be precipitated by a solution of tin, it is of importance, that the tin be in a fixed state of oxidation, otherwise the product will vary both in its nature and quantity. A uniform solution may always be obtained by dissolving slips of tin in muriatic acid at 12° [1·089], filtering, evaporating to the point of crystallization, dissolving the crystals in pure water, and filtering again. Part of this solution should immediately be mixed with the liquid muriate of gold; and the union of the two salts produces a precipitate, which should be increased by adding fresh quantities of the muriate of tin, as long as any thing is thrown down; after which the precipitate is to be washed, dried, and powdered. The quantity obtained appears to depend on the quantity of water added to the solutions of gold and tin. The more they are diluted, the more tin is thrown down. One drachm of gold, the solution of which was mixed with ten quarts of water, mixed with a very dilute solution of tin, yielded near five drachms and half of a very fine purple precipitate.

Preparation of the solution.

Difference between the two precipitates.

It does not appear to be a matter of indifference which of these two precipitations is used. When metallic tin is employed, the precipitate is brown; and the gold, if not in the metallic state, is nearly approaching it. On the contrary, the precipitate produced by muriate of tin at a minimum of oxidation is of a deep purple colour; and, though it contains a little metallic gold, has much more of the oxides of gold and of tin; whence, it is obvious, the efficacy of the two preparations cannot be the same.

Muriate of gold very deliquescent.

The muriate of gold is so greedy of moisture, that it soon deliquesces, whence it can be employed only in the liquid state; and, as its great causticity renders even this difficult, Dr. Chrestien thought of uniting it with the muriate of soda; thus producing a triple muriate, less deliquescent, and less caustic.

Triple muriate of gold and soda.

For this purpose a solution of muriate of gold in distilled water, obtained as described above, is to be employed; and it is particularly important, that this salt has not an excess of

acid.

acid. Into this solution is to be poured an aqueous solution of pure decrepitated muriate of soda, so as to combine an equal quantity of the dry salt with the gold dissolved. The two solutions being mixed, the fluid is to be evaporated by a gentle heat in a glass capsule, taking care to stir it well toward the end of the process. When the mass is sufficiently dry, it is to be powdered while hot in a glass or stone mortar; and the powder is to be kept from moisture, which it attracts in a slight degree.

Management of the fire important.

In this preparation the management of the fire is of great importance: for, if the desiccation of the salt be not carried far enough, it will contain too much acid; and, if it be urged too far, it will be in part decomposed, and mixed with a little gold.

Dr. Chrestien's mode of employing these preparations.

The enlightened physician, who extols the use of these preparations, employs them externally and internally; but recommends them to be mixed with other substances, lest their action should be too violent, if given alone. Thus for a long time he did not give the triple muriate of gold and soda otherwise than mixed with twice its weight of a powder composed of starch, charcoal, and the lake used by painters. As the alumine of the last however might take up a portion of the muriatic acid, and the charcoal might revive the gold, Dr. Chrestien changed this powder for that of liquorice root, orris root, &c.

Beside this he joined the compounds of gold with extracts of the attenuant plants; sugar with which he forms lozenges; sirups, in which he dissolves them, &c. He mixes them also with Galen's cerate, when he wishes to promote suppuration, and with lard, when he would employ them in frictions on the soles of the feet after the method of Cyrillo.

This faulty.

The writers of the present article do not approve the combination of the preparations of gold with these different substances, as all vegetable and animal substances, dissolved or not, revive gold from its acid solution. They recommend them to be given alone, or dissolved in distilled water: or at least, if they must be mixed, to mix them as short a time as possible before they are used.

Instances of the efficacy

In this way Dr. Duportal asserts that he has found good effects from them in siphylitic complaints. In a chancre corroding

corroding one of the corpora cavernosa he found them of real advantage: but the most striking instance of their efficacy was in a cancerous ulcer, that had destroyed the upper lip, attacked the soft parts of the nose and left cheek, destroyed the square bones [os carrés], and rendered the maxillary bone carious. Being called to a consultation with Dr. Payen on this very serious case, in which all the common methods had been tried in vain, Dr. Duportal hoped to oppose the progress of the disease by the use of Dr. Chrestien's medicine assisted by attenuant extracts. In consequence the patient was directed daily to rub into the gums the triple muriate of gold and soda; and to take oxide of gold precipitated by potash, with pills composed of the extracts of white henbane, hemlock, and sharp-pointed toadflax. The ulcer was daily washed with Sydenham's liquid laudanum, sprinkled over with powder of red bark and camphor, and dressed with a digestive in which oxide of gold was mixed. Under this treatment, which has been continued two months gradually increasing the dose of the substances, the ulcer has assumed a promising appearance; the carious points have disappeared; the suppuration furnishes laudable pus in moderate quantity; the patient daily improves in flesh and strength; and there is every reason to believe, that this evident melioration will continue. That it cannot be ascribed to the means employed in conjunction with the preparations of gold is evident, for they had been used previous to these without effect.

of the preparations of gold.

IV.

Experiments on the Existence of Water in Muriate of Ammonia formed by the Combination of Muriatic Acid and Ammoniacal Gasses. By Mr. JOHN MURRAY, *Lecturer on Chemistry, Edinburgh.*

To Mr. NICHOLSON.

SIR,

I Have beeu prevented by different circumstances from bestowing any attention until lately on the objections, which have

Mr. Murray's experiment

have been made to the experiment in my last communication proving the existence of water in muriatic acid gas. This I have little reason to regret, as the deficiency has been amply supplied by the candid communication from Dr. Bostock and Dr. Traill in the supplement to your last volume. From the care with which their experiment appears to have been conducted, it must be regarded as nearly decisive of the question at issue; and the result coinciding so exactly with that which I had stated to be obtained, while it is at variance with that affirmed by Messrs. Davies, I might probably spare myself the task of taking any notice of the observations of my opponent. As I have executed some experiments however, which occurred to me on this subject, a brief account of the results may not be unacceptable to your chemical readers.

confirmed by Drs Bostock and Trail.

Admissions of its opponents.

It has been admitted, that the experiment which I have brought forward, if accurate, is conclusive on the subject of this discussion. It has also been admitted, that when the experiment is performed in the manner I described, the result is that which I stated to be obtained—a sensible and even a considerable portion of water being produced, when the salt formed by the combination of muriatic acid and ammoniacal gasses is exposed to heat. But to obviate the conclusion from this it has been asserted, that the salt, while it is transferring from the vessel in which it is formed to that in which it is heated, absorbs water from the atmosphere, and that this is the source of the water it affords. This explanation has been given on the authority of Mr. Davy, who, it it stated, performed the experiment without obtaining water when this source of fallacy was avoided. And Mr. J. Davy, who it seems was disposed to doubt of the accuracy of my experiment before he knew of this mode of accounting for its result, states, that he was informed of it by his brother; who farther told him, that, if he heated the salt without exposure to the air, he would obtain no water. He accordingly made the experiment as it is described in your Journal (vol. XXXI, p. 314), and found no water to be produced; but when the experiment was made in the manner I had performed it, "water in no inconsiderable quantity was evolved:" and now, it is added, "we have a demonstration, that the water

Attempt to obviate the conclusion from it.

librated

librated in Mr. Murray's experiment was not derived from the muriatic gas, but from the atmosphere."

That muriate of ammonia attracts water from the air should have been proved.

It might have been expected, that the first step these gentlemen would have taken, when they assigned this as the source of the water obtained, would have been to prove its reality; and to show by experimental evidence, that the salt on which they operated has the power of attracting water from the atmosphere. No such evidence however is given; but the existence of this property is inferred from the result of an experiment, which may have arisen from causes altogether different.

The experiment of Messrs. Davies not conclusive.

Admitting for a moment the accuracy of their experiment, the obtaining water when the salt is heated after exposure to the air, while none is obtained when it is heated without this exposure, is no proof, that the water in the former case has been absorbed from the atmosphere; for in making the experiment in these two modes, the sole difference is not the admission or exclusion of the air, nor is the sole operation of the air that of affording moisture; there are other circumstances of difference equally important, and which it is easy to perceive must influence the result.

Principal difficulty in the original experiment.

Thus the principal difficulty in the original experiment, so as to render it conclusive, arises from the volatility of the ammoniacal salt, and the inconsiderable interval of temperature between that point at which any water it may contain can be separated from it by heat, and that point at which the salt itself will pass into vapour. In consequence of this it must require a nice regulation of temperature to obtain the one effect without the other; and from this very circumstance, even had water not been obtained in the experiment as I first performed it, it could not have been affirmed, that it did not exist in the salt.

This particularly exists in their mode of executing it;

Now this difficulty it is obvious is much greater, when heat is applied to a thin layer of salt encrusted over the whole internal surface of a retort, than when it is applied to the same quantity of salt collected in mass at the bottom of a retort; and it must indeed be nearly impracticable to apply the heat in the former case with such a precise adaptation to the relative volatilities of the water and the salt, as to expel the former without volatilizing the latter. If the heat therefore is kept sufficiently low not to volatilize the salt, and especially if care is taken to keep it

 still

still lower than this, it is possible, that there may b no apparent production of water. If the salt too has any power of absorbing water, inferior even to what these gentlemen suppose, it is evident, that the portion of it in the upper part and curvature of the neck of the retort must absorb the small portion of water, that may be volatilized by a moderate heat applied to the salt at the bottom, or in the body of the retort; and according therefore to the assumption they themselves maintain, no water ought to appear in this mode of making the experiment, even though the salt may contain it. Farther, if any pressure is present in consequence of the arrangement by which the air is excluded, (and Mr. Davy's experiment is not sufficiently described, to enable us to determine whether this were the case or not,) this must retard or prevent the separation of the water. And lastly, when the air is excluded, that agency of it by which it promotes the transition of every substance into vapour by heat, lately so well illustrated by Gay-Lussac*, is prevented from operating; and the same result with regard to the expulsion of water in vapour from any matter containing it cannot be obtained, as when a communication with the atmosphere is preserved. It was to obviate some of these circumstances, that I performed the experiment in the manner in which it was originally executed. All of them however are neglected by Messrs. Davies, though it is obvious, that their influence must be important; and to account for the result they are said to have obtained, the *supposition* is introduced of the salt attracting water from the atmosphere, without any experimental evidence being given, that it has any such power.

and ought, on their own principle, to prevent the appearance of water.

Other objections to their experiment.

The cause they assume for the appearance of water fallacious.

I was satisfied prior to any experimental investigation, that the cause thus hypothetically assigned is altogether fallacious. When a soluble substance attracts water from the atmosphere, it continues to attract it, until it becomes humid, and is at length dissolved. This is the case with potash, muriate of lime, acetate of potash, and indeed every salt known to absorb water from the air; and it follows from the very property itself. The deliquescent substance imbibes water in consequence of the strong attraction it has to it; and this attraction must continue to operate, until an equilibrium between

* Memoires D'Arcueil, tom. 1, p. 204.

it

it and the force of cohesion is attained; and in a soluble substance therefore must continue until it is dissolved. No such property belongs however to muriate of ammonia; every chemist knows, that in an atmosphere in a common state of dryness it is not deliquescent, but remains dry for any length of time. There is no reason to believe, that it is capable of absorbing water short of that quantity, which shall produce sensible humidity; and it is altogether an extravagant assumption, that it can absorb water with such rapidity, as in a few minutes to imbibe that considerable quantity which it yields when exposed to heat. With regard to any hygrometric effect from the loose pulverulent state of the salt, it is not less extravagant to suppose, that it could operate so speedily, or to such an extent as is necessary to account for the result of the experiment; or that it could operate after the salt had been heated, so as to enable it to afford the quantity which even then it yields*.

The salt cannot act hygrometrically.

Fortunately the determination of this point is not attended with any peculiar difficulty. It may be ascertained by experiment, whether the salt does absorb water or not from the air, and whether the water which it yields when heated is derived from this source.

The point easily determined.

I first performed the experiment of heating the salt without its having been exposed to the air. In a small retort, over dry quicksilver, I combined in successive portions 25 cubic inches of ammoniacal gas, which had been dried by exposure to lime, with muriatic acid gas, which had been exposed to muriate of lime, adding at the end an excess of ammoniacal gas to fill the retort. The retort was then turned over in such a manner, that the extremity of its neck was kept under the quicksilver, and an inverted jar filled with quicksilver was placed over it. The body of the retort being surrounded with sand, heat was applied by an Argand's lamp

Experiment described.

* After the salt has afforded water by being heated, I had found it to afford an additional portion, when it is exposed to a stronger heat. It is also stated in your Journal (vol. XXXI, p. 237,) that water may be obtained from the salt successively by heating it repeatedly, if it is exposed to the atmosphere for a few minutes each time; and it is added, that in this way I might have obtained water to the amount of thrice the weight of the salt. No doubt, if the salt thus absorbs water, it may continue to afford it to fifty times its weight.

with

with a double wick; and afterward the heat was applied to the naked retort. In about ten minutes moisture appeared in the neck, and continued to accumulate, so that a dew covered a space of about two inches in length, and united into small globules. At the end of the experiment the salt was found to be sublimed entirely into the upper part of the body of the retort, and the curvature of its neck.

This repeated in different ways.

This experiment was repeated under different forms. In one, the two gasses were combined in small successive portions in the upper part of a long glass tube over dry quicksilver. The combination being completed, the tube, which had such a degree of curvature towards the middle of it, that, when placed horizontally, its extremity could be kept immersed in quicksilver, was turned over into this horizontal position, and ignited charcoal was placed around part of it, so that heat was communicated to its closed extremity, where the salt was collected, sufficient to volatilize it. Moisture in this case also was condensed on the sides of the tube. And in all the experiments, which were performed, sensible quantities of water were obtained*.

Exposure of the salt to the air for 15′ had no effect on the result.

I next repeated the experiment in another form. The salt was formed by the combination of the gasses in the retort, or in the tube as before. But previous to applying heat to it, it was left exposed for fifteen minutes to the air. The extremity of the neck of the retort or of the tube being then immersed in quicksilver, heat was applied as before, and as nearly as possible in the same manner, and to the same extent. The condensation of moisture was soon apparent, but the quantity was not greater, so far as could be estimated, than was obtained from the salt heated without having been exposed to the atmosphere. This exposure therefore, when the other circumstances of the experiment were the same, had no influence on the result.

Experiment to ascertain, whether the

I next proceeded to ascertain by more direct experiment, whether the salt does attract any moisture or not from the atmosphere. A glass bottle of the capacity of six cubic

* A few of the words toward the close of this paragraph were so obliterated by the seal, that it was necessary to supply them by conjecture. C.

inches

inches was filled with dry ammoniacal gas; muriatic acid gas, which had been exposed to muriate of lime, was added to it over dry quicksilver, and successive portions of the two gasses were introduced, until about 24 cubic inches of muriatic acid gas had been combined, the salt formed condensing over nearly the whole internal surface of the bottle. It was then filled with dry ammoniacal gas, and, a stopper fitted to it being introduced under the quicksilver, it was removed, and accurately weighed in a very sensible balance. The stopper was removed for a moment to allow the ammoniacal gas to escape, and atmospheric air to enter in its place. The bottle gained immediately 0·6 of a grain in weight from the substitution of the one air for the other. The stopper was again removed, and was placed in the scale, and no farther weight was gained. At the end of five minutes it remained perfectly the same, at the end of ten minutes it still remained exactly balanced; at fifteen minutes it was still stationary; at twenty minutes there appeared to be a very slight indication of increase of weight in the bottle; at the end of half an hour from the commencement of the weighing this was more apparent, and amounted to about 2° on the scale of the balance; at the end of an hour it had increased to 5°; and at the end of two hours to 10°. This total increase was found equal to 0·5 of a grain; the salt collected from the bottle weighed 13 grs; being wrapped loosely in paper it remained perfectly dry; and after two days its weight was found to be as nearly as possible the same.

salt attract moisture,

The result of this experiment proves, that muriate of ammonia formed by the combination of muriatic acid and ammoniacal gasses absorbs no moisture from the air, or at least none which can account for the production of water from it when it is exposed to heat. Two or three minutes are sufficient to transfer it from the vessel in which it is formed to that in which it is heated. During this time, and even for 15 minutes, it does not absorb the smallest portion of moisture, for it gains no weight whatever; at the end of an hour the increase of weight was not more than 0·25 of a grain; and the total increase at the end of two hours was not equal to one fourth of the weight of water, which the salt yields by heat. Nor is there any certainty,

proves that it does not.

that

that any part of this increase of weight arose from the absorption of humidity. It was more probably owing to the ammoniacal gas not being entirely expelled, when the stopper was first withdrawn, but being retained by a slight force in the interstices of the salt, and being only slowly detached by the atmospheric air. It was also not a uniform result, and in a subsequent experiment, in which the gasses were combined in a globe furnished with a stop cock, there was rather a very slight diminution of weight. The two experiments therefore are conclusive in proving, that this salt does not absorb humidity from the atmosphere.

Farther proof, that it does not.

One other experiment afforded a very satisfactory demonstration, that the muriate of ammonia formed by the combination of its constituent gasses has no power of absorbing water either by chemical attraction, or by what is named hygrometric affinity. In the experiment in which water was expelled from the salt by heat, the mouth of the retort or of the tube was closed, and the moisture condensed on its sides was thus submitted to the action of the salt in the most favourable manner; it must therefore have been quickly absorbed, had the salt any power of attracting water, such as has been supposed; but it remained without any diminution for a number of hours; and even after twenty-four hours the globules of water remained apparent. It is impossible to conceive a result, which can prove more satisfactorily that the salt has no such power.

These experiments conclusive.

These experiments then I consider as conclusive in refuting the supposition, by which it has been attempted to account for the water afforded by this salt when it is heated, that it is water which it has absorbed from the atmosphere. It is found, that it affords water when it is heated without having been exposed to the atmosphere; that the quantity it does afford is, as nearly as can be estimated, as great as that which it yields when it has been previously exposed; that it does not absorb humidity from atmospheric air in its usual state of dryness; and that it does not even reabsorb the water, which has been expelled from it by heat. The original experiment then, I trust, I may consider as remaining in full force, and as affording a conclusive proof of the existence of water in muriatic acid gas, and a proof of course of the falsity of the hypothesis which Mr. Davy has endeavoured to defend. I

Remarks on Mr. Davy's last communication.

I have no wish to enter on the discussion of the remaining observations of Mr. J. Davy in his last communication. I only feel myself called on to make one or two remarks on his assertion with regard to the accuracy of his own and his brother's experiments, and the inaccuracy of mine. He thinks proper to say, that all my experiments have been found to be incorrect; that I have advanced no arguments, that have not been answered, no experiments, the accuracy of which has been admitted. I shall merely meet these assertions by recalling in a very brief manner to the notice of your readers, the fact originally established by my experiments, the various kinds of denial which Messrs. Davies gave to them, and the admission which they have at length been compelled to yield to them.

The conversion of carbonic oxide into carbonic acid by oximuriatic gas first denied by Messrs. Davies

At the commencement of this controversy I had affirmed, that, when dry carbonic oxide, hidrogen, and oximuriatic acid gasses are submitted to mutual action, the carbonic oxide disappears, and carbonic acid is obtained. They opposed to this the supposition, that the conversion of carbonic oxide into carbonic acid was owing to the decomposition of water admitted to examine the product; or to the presence of atmospheric air, or the intermixture of a compound of oximuriatic acid and oxigen in the oximuriatic gas I employed: and they affirmed, that, when these sources of fallacy were avoided, and particularly when ammonia was employed to condense the product, the carbonic oxide remained unchanged, and no carbonic acid was formed*. Though satisfied of the futility of these suppositions, I repeated the experiment with this variation; and still obtained the same result, the disappearance of the carbonic oxide, and the production of carbonic acid when the salt formed by the ammonia was decomposed by an acid. Still Messrs. Davies attempted to deny these results; and to support them in this denial they had recourse to some very singular methods†. They repeated my experiment to prove it incorrect, but instead of executing it in the manner in which I had performed it, as common candour, and common accuracy required, they diminished

* Journal, vol. XXVIII, p. 209, &c. vol. XXIX, p. 235.

† Ibid, vol. XXIX, p. 42, 188.

the

the proportion of hidrogen to less than one half, (using four measures to ten of carbonic oxide instead of equal measures) thus not only altering it in a material circumstance, but withdrawing as far as possible the very circumstance, which I had held essential to its success. And to prove, that the results of my experiments had arisen from the presence of atmospheric air, or of moisture in the gasses, they brought forward an experiment, in which both these were allowed to operate, instead of being excluded; and then contended, that the partial conversion of carbonic oxide into carbonic acid, which they did obtain, arose from the very sources of fallacy, which it ought to have been their care to exclude, but which they thus chose to admit.

then admitted by Mr. J. Davy,

At length, after all these attempts, Mr. J. Davy announced the discovery of a new gas, a compound as he supposed of oximuriatic acid and carbonic oxide, by the operation of which he farther supposed the formation of carbonic acid might be accounted for in conformity to his brother's hypothesis; and then he at once admitted what I had uniformly asserted, and what he and his brother had before as steadily denied, that the carbonic oxide disappears, and that carbonic acid is obtained, when the ammoniacal salt is decomposed by an acid. "Repeating my experiment on the exposure of the three gasses to light," he detected, "after the addition of ammonia, no traces of carbonic oxide;" and he perceived "an effervescence of the "ammoniacal salt with nitric acid," which effervescence he farther admits to be owing to carbonic acid*. These are the precise results I had obtained. How then can Mr. J. Davy venture to assert, that there are no experiments of mine the accuracy of which has been admitted? or how does he reconcile the admissions he now makes with the former positive assertions by himself and his brother, that, in the mutual action of these three gasses, the carbonic oxide remains unchanged, and no carbonic acid is formed?

but said to be effected indirectly.

There is one mode indeed, by which he throws some obscurity over this result of the controversy. He maintains, that the production of carbonic acid in these experiments is effected in an indirect mode; the oximuriatic acid

* Journal, vol. XXX, p. 30, vol. XXXI, p. 311.

and

and the carbonic oxide he supposes combine and form an acid gas, which unites with the ammonia; and when the salt formed by this union is decomposed by an acid, this gas he imagines decomposes water, and forms muriatic and carbonic acids. I have already given my reasons, which I need not repeat, for considering every thing relating to this gas as at present in the highest degree doubtful; and with regard to its supposed agency in decomposing water I also pointed out to him an inconsistency in his statement, which he calls imaginary, but which is real, and remains still unexplained. While he supposed, by a very circuitous mode of reasoning, that it decomposes water, I observed to him, that he had not ascertained the fact; and that he had even stated as one of the properties of this gas, that it is "very slowly absorbed by water," a statement directly at variance with the supposition, that it decomposes water; for the result of this decomposition must be an instantaneous reduction of volume by the absorption of the muriatic acid, which is one of its products, and a rapid absorption of the carbonic acid, which is its other product. He has accordingly since stated, that the gas, immediately on coming into contact with water, is decomposed, and converted into carbonic and muriatic acid gasses: and he adds "in my first notice of the gas I mentioned its being apparently slightly absorbed by water only among its most obvious qualities, those which made the first impression on me, and led me to consider it as a new substance." But he forgets to explain how in a result so obvious, and in which there appears to be no room for fallacy, he should first have found, that this gas is very slowly absorbed by water; and afterward, when I had pointed out to him that this was incompatible with his supposition that it decomposes water, that he should have discovered, that immediately on coming into contact with water it is resolved into muriatic and carbonic gasses, which must be quickly absorbed.

Inconsistency in his statement still unexplained.

These are points however, on the consideration of which it is not necessary to enter. Whatever importance may be attached to them as connected with the discussion on the nature of oximuriatic acid, they are of no importance in regard

Farther remarks on the subject.

Farther remarks on the subject.

regard to the ultimate results of the experiments. The question in this point of view is not how carbonic acid is formed, whether directly or indirectly, but whether it is formed at all. Messrs. Davies affirmed, in contradiction to what I stated, that *it is not formed*. Mr. J. Davy now admits, that *it is formed*; and he may account as he is able for these opposite assertions: or, to remove the slight ambiguity which arises from involving the statement of the fact of the production of carbonic acid with the inquiry as to the manner in which it is produced, let the question be restricted to the effect on the carbonic oxide. I had uniformly affirmed, that it disappears. Messrs. Davies asserted, as the results of repeated experiments, that it remains unchanged*. But Mr. J. Davy now tells us, that it does disappear, so that no traces of it can be discovered after the addition of ammonia. On this I shall offer no comment, but rest satisfied with the simple statement of the fact; and if Mr. J. Davy after this thinks proper to repeat his assertions on the accuracy of his and his brother's experiments, and on the inaccuracy of mine, I shall certainly not feel it incumbent on me to take any notice of them. Allow me to add, that I regret having been compelled to make these observations; but I conceive I should be wanting in what I owe to myself, did I not repel assertions so injurious and unwarranted; and I believe I have done so in terms less severe than what the occasion might justify.

Sir H. Davy's opinion not a theory.

What farther relates to the general reasoning on this controversy, I leave altogether to the judgment of your readers. Mr. J. Davy "confesses himself totally at a loss to understand" how I have shown what he calls the theory of his brother (though strictly speaking it is entitled to neither of these appellations) to be an hypothesis: he still considers it he informs us as an expression of facts in all its essential parts, to the exclusion of hypothesis; and I have advanced it seems no arguments, that have not been answered.

I had supposed Mr. J. Davy to have been peculiarly unfortunate in his attempts to answer these arguments; and had

* Journal, vol. XXVIII, p. 201, vol. XXIX, pp. 42, 235.

had supposed the question, whether this doctrine is a theory or an hypothesis, to have been brought into that point of view, that it was too obvious to bear any farther discussion. I may be mistaken in this; but still I cannot persuade myself, that there is any necessity for my entering on any recapitulation or extension of the arguments I have employed. With many of your readers they may have more weight than with my opponents; and my want of success in the latter respect, it is possible, may be owing not so much to deficiency in the argument, as in the person to whom it is addressed; for one who, like Mr. J. Davy, could not distinguish between an inference from a fact, and the expression of the fact itself*; who could confound an insulated fact, which his hypothesis did not explain, with an ultimate fact of which no explanation was to be expected, and who could call this fact one of the axioms of the science†; can hardly be expected, even with the most candid dispositions, to discriminate very accurately between the nicer limits, by which theory and hypothesis are defined. I shall not attempt therefore to convince this gentleman, but shall leave him in full possession of the belief (if he seriously entertains it) that he has answered all my arguments, refuted all my experiments, and established his brother's opinion as a genuine theory.

Relation of muriatic acid to water not peculiar.

I shall only add, that the late progress of chemical discovery has shown, that there is nothing peculiar in the relation of muriatic acid to water, such as is maintained in the common doctrine. The able researches of Gay-Lussac and Thenard and of Berthollet have shown, that all the more powerful acids, the sulphuric, nitric, phosphoric, and fluoric, contain combined water, from which they cannot be obtained free in an insulated state. Those of your readers, who feel an interest on this subject, will find a summary of these researches in the supplement to the second edition of my System of Chemistry, lately published.

I have the honour to be,

Your most obedient servant,

Edinburgh, May 31, 1812. JOHN MURRAY.

* Journal, vol. XXIX, pp. 39, 195.

† Ibid, vol. XXVIII, pp. 199, 302.

V.

V.

METEOROLOGICAL JOURNAL.

1812.	Wind	Pressure. Max.	Min.	Med.	Temperature. Max	Min	Med.	Evap.	Rain	
5th Mo.										
May 4	N E	29·86	29·78	29·820	63	38	50·5	—		☽
5	E	30·01	29·86	29·935	64	40	52·0	—		
6	E	30·01	29·98	29·995	60	42	51 0	—		
7	E	29·94	29·86	29·900	58	45	51·5	·70		
8	S E	29·86	29·73	29·795	76	51	63·5	—		
9	S W	29·78	29·68	29·730	72	53	62·5	·90	—	
10	W	29·82	29·56	29·690	64	56	60·0	—	·20	
11	N W	29·56	29·54	29·550	65	49	57·0	—	·02	●
12	S W	29·53	29·51	29·520	65	44	54·5	—	·13	
13	S	29·56	29·50	29·530	60	40	50·0	·44	·15	
14	S	29·75	29·56	29·655	58	40	49·0	—	—	
15	N E	29·95	29·75	29·850	57	43	50·0	—	·10	
16	N	30·00	29·95	29·975	62	45	53·5	·36	2	
17	N E	29·95	29·87	29 910	53	45	49·0	—	7	
18	E	29·87	29·80	29·835	63	48	55·5	—		☾
19	E	29 80	29·63	29·715	66	53	59·5	—	·44	
20	S W	——	——	——	65	53	59·0	—	8	
21	Var.	29·94	29·63	29·785	61	45	53.0	—	·60	
22	N W	30·18	29 98	30·080	52	35	43·5	—		
23	E	30·27	29·98	30·125	61	40	51·5	·32		
24	S E	30·27	30·11	30·190	57	52	54·5	—	4	
25	S W	30·11	29·98	30·045	62	53	57·5	—		
26	S W	29·98	29·55	29·765	72	55	63·5	·43		○
27	S	29·59	29·55	29·570	71	51	61·0	—		
28	S E	29·69	29·59	29·640	69	54	61·5	—	·14	
29	S W	29·84	29·69	29·765	72	53	62·5	—	·23	
30	S	29·76	29·74	29·750	67	52	59·5	·65		
31	S	29·75	29·72	29·735	65	54	59·5	—	·10	
6th Mo.										
June 1	S W	29·95	29·72	29·835	60	46	53·0	28	4	
		30·27	29·50	29.810	76	35	53·46	4·08	2·36	

The observations in each line of the Table apply to a period of twenty-four hours beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

NOTES.

NOTES.

Fifth Month. 4, 5, 6, Much dew. 7. Windy. 8. Windy: cirro-cumulus and cumulo-stratus: wind S. above: thunder clouds: the evening twilight was luminous and coloured: the clouds dispersing, and scattered in loose flocks over the rich ground of the western sky, presented a striking appearance. 9. Shower very early: Wind S. cirrus, cirro-cumulus: evening, much wind. 10. A. m. overcast: a gale from the W. with much cloud: showers: p. m. clear and pleasant; 11. A shower early: cumulo-stratus prevails. 12. Showers. 13. A thunder shower, with hail, about 3 p. m. 14. Showers. 15, 17. Cloudy, windy. 18. A. m. small rain: wind N. gentle: p. m. sunshine. 19. A. m. Wind E. pretty strong: clouds of different kinds, with haze above: p. m. thunder clouds: in the evening came on a violent thunder storm, which lasted several hours; it was chiefly to the S. and W. The appearances were very similar to those of the destructive hail storm, which occurred here in the same month, and on the same day of the month, and nearly at the same time of the day, in 1809: sheets of blue and white lightning came in quick succession, with an almost continual rolling of thunder. We had however no hail (being only on the flank of the storm) but sudden and heavy showers of warm rain; which was of the same amount in the upper as in the lower gauge. At 11, p. m. wind N. E. it still lightned far in the N. 20. A. m. wind W. cloudy and misty. 23. About noon, during a shower, it thundered to the southward. 29. A little thunder to the S. W. about 4 p. m. with a few drops: wet night. 31. An electric shower about 9, a. m. Nimbi: windy night.

RESULTS.

Winds variable.

Barometer: highest observation 30·27 inches; lowest 29·50 inches; Mean of the period 29·810 inches.

Thermometer: highest observation 76°; lowest 35°; Mean of the period 55·46°.

Evaporation 4·08 inches. Rain 2·36 inches.

PLAISTOW,
Sixth Month, 1812.

L. HOWARD.

VI.

VI.

Account of the Pitch Lake of the Island of Trinidad. By NICHOLAS NUGENT, *M. D., Hon. Mem. of the Geol. Soc.**

Visit to the pitch lake of Trinidad.

BEING desirous to visit the celebrated Lake of Pitch, previously to my departure from the island of Trinidad, I embarked with that intention in the month of October, 1807, in a small vessel at Port Spain. After a pleasant sail of about thirty miles down the gulf of Paria, we arrived at the point la Braye, so called by the French from its characteristic feature. It is a considerable headland, about eighty feet above the level of the sea, and perhaps two miles long and two broad. We landed on the southern side of the point, at the plantation of Mr. Vessigny: as the boat drew near the shore, I was struck with the appearance of a rocky bluff or small promontory of a reddish brown colour, very different from the pitch which I had expected to find on the whole shore. Upon examining this spot, I found it composed of a substance coresponding to the porcelain jasper of mineralogists, generally of a red colour, where it had been exposed to the weather, but of light slate blue in the interior: it is a very hard stone with a conchoidal fracture, some degree of lustre, and is perfectly opake, even at the edges: in some places, from the action of the air, it was of a reddish or yellowish brown, and an earthy appearance. I wished to have devoted more time to the investigation of what in the language of the Wernerian school is termed the geognostic relations of this spot, but my companions were anxious to proceed. We ascended the hill, which was entirely composed of this rock, to the plantation, where we procured a negro guide, who conducted us through a wood about three quarters of a mile. We now perceived a strong sulphureous and pitchy smell, like that of burning coal, and soon after had a view of the lake, which at first sight appeared to be an expanse of still water, frequently interrupted by clumps of dwarf trees, or islets of rushes and shrubs: but on

Porcelain jasper.

The lake described.

* Trans. of the Geol. Society, vol. I, p. 69.

on a nearer approach we found it to be in reality an extensive plain of mineral pitch, with frequent crevices and chasms filled with water. The singularity of the scene was altogether so great, that it was some time before I could recover from my surprise so as to investigate it minutely. The surface of the lake is of the colour of ashes, and at this season was not polished or smooth so as to be slippery; the hardness or consistence was such as to bear any weight, and it was not adhesive, though it partially received the impression of the foot; it bore us without any tremulous motion whatever, and several head of cattle were browsing on it in perfect security. In the dry season however the surface is much more yielding, and must be in a state approaching to fluidity, as is shown by pieces of recent wood and other substances being enveloped in it. Even large branches of trees, which were a foot above the level, had in some way become enveloped in the bituminous matter. The interstices or chasms are very numerous, ramifying and joining in every direction, and in the wet season being filled with water, present the only obstacle to walking over the surface; these cavities are generally deep in proportion to their width, some being only a few inches in depth, others several feet, and many almost unfathomable: the water in them is good and uncontaminated by the pitch; the people of the neighbourhood derive their supply from this source, and refresh themselves by bathing in it: fish are caught in it, and particularly a very good species of mullet. The arrangement of the chasms is very singular, the sides, which of course are formed of the pitch, are invariably shelving from the surface, so as nearly to meet at the bottom, but then they bulge out towards each other with a considerable degree of convexity. This may be supposed to arise from the tendency in the pitch slowly to coalesce, whenever softened by the intensity of the Sun's rays. These crevices are known occasionally to close up entirely, and we saw many marks or seams from this cause. How these crevices originate it may not be so easy to explain. One of our party suggested, that the whole mass of pitch might be supported by the water, which made its way through accidental rents, but in the solid state it is of greater specific gravity than water, for

Branches of trees above its level enveloped with pitch.

Chasms in the pitch filled with good water, containing fish.

Islets in the lake.

several bits thrown into the pools immediately sunk*. The lake (I call it so because I think the common name appropriate enough) contains many islets covered with long grass and shrubs, which are the haunts of birds of most exquisite plumage, as the pools are of snipe and plover. Alligators are also said to abound here, but it was not our lot to encounter any of these animals. It is not easy to state precisely the extent of this great collection of pitch; the line between it and the neighbouring soil is not always well defined, and indeed it appears to form the substratum of the surrounding tract of land. We may say, however, that it is bounded on the north and west sides by the sea, on the south by the rocky eminence of porcelain jasper, before mentioned, and on the east by the usual argillaceous soil of the country; the main body may perhaps be estimated at three miles in circumference; the depth cannot be ascertained, and no subjacent rock or soil can be discovered.

Its extent.

Vegetation on it where there is a thin soil.

Where the bitumen is slightly covered by soil, there are plantations of cassava, plantains, and pine apples, the last of which grow with luxuriance, and attain to great perfection. There are three or four French and one English sugar estates in the immediate neighbourhood; our opinion of the soil did not, however, coincide with that of Mr. Anderson, who, in the account he gave some years ago, thought it very fertile. It is worthy of remark, that the main body of the pitch, which may properly be called the lake, is situate higher than the adjoining land, and that you descend by a gentle slope to the sea, where the pitch is much contaminated by the sand of the beach. During the dry season, as I have before remarked, this pitch is much softened, so that different bodies have been known slowly to sink in it; if a quantity be cut out, the cavity left will be shortly filled up: and I have heard it related, that when the Spaniards

The surface of the pitch higher than that of the neighbouring land.

Much softened in the dry season.

* Pieces of asphaltum are, I believe, frequently found floating on the Dead Sea in Palestine, but this arises probably from the extraordinary specific gravity of the waters of that lake, which Dr. Marcet found to be 1·211. Mr. Hatchett states the specific gravity of ordinary asphaltum to vary from 1·023 to 1·165, but in two varieties of that of Trinidad it was as great as 1·336 and 1·744, which led Mr. Hatchett to form a conjecture, which I shall afterwards notice.

niards undertook formerly to prepare the pitch for economical purposes, and had imprudently erected their cauldrons on the very lake, they completely sunk in the course of a night, so as to defeat their intentions. Numberless proofs are given of its being at times in this softened state: the negro houses of the vicinage, for instanec, built by driving posts in the earth, frequently are twisted or sunk on one side. In many places it seems to have actually overflown like lava, and presents the wrinkled appearance which a sluggish substance would exhibit in motion.

This substance is generally thought to be the asphaltum of naturalists: in different spots however it presents different appearances. In some parts it is black, with a splintery conchoidal fracture, of considerable specific gravity, with little or no lustre, resembling particular kinds of coal, and so hard as to require a severe blow of the hammer to detach or break it; in other parts, it is so much softer, as to allow one to cut out a piece in any form with a spade or hatchet, and in the interior is vesicular and oily; this is the character of by far the greater portion of the whole mass; in one place, it bubbles up in a perfect fluid state, so that you may take it up in a cup, and I am informed, that in one of the neighbouring plantations there is a spot where it is of a bright colour, shining, transparent, and brittle, like bottle glass or resin. The odour in all these instances is strong and like that of a combination of pitch and sulphur. No sulphur however is any where to be perceived, but from the strong exhalation of that substance and the affinity which is known to exist between the fluid bitumens and it, much is, no doubt, contained in a state of combination; a bit of the pitch held in the candle melts like sealing wax, and burns with a light flame, which is extinguished whenever it is removed, and on cooling the bitumen hardens again. From this property it is sufficiently evident, that this substance may be converted to many useful purposes, and accordingly it is universally used in the country wherever pitch is required; and the reports of the naval officers who have tried it are favourable to its more general adoption; it is requisite merely to prepare it with a proportion of oil, tallow, or common tar, to give it a sufficient degree of fluidity.

The substance varies much.

It smells of sulphur;

melts in the flame of a candle, and hardens on cooling.

Used as pitch.

ity. In this point of view, this lake is of vast national importance, and more especially to a great maritime power. It is indeed singular, that the attention of government should not have been more forcibly directed to a subject of such magnitude: the attempts that have hitherto been made to render it extensively useful have for the most part been only feeble and injudicious, and have consequently proved abortive. This vast collection of bitumen might in all probability afford an inexhaustible supply of an essential article of naval stores, and being situate on the margin of the sea could be wrought and shipped with little inconvenience or expense*. It would however be great injustice to Sir Alexander Cochrane not to state explicitly, that he has at various times, during his long and active command on the Leeward Island station, taken considerable pains to insure a proper and fair trial of this mineral production for the highly important uses of which it is generally believed to be capable. But whether it has arisen from certain perverse occurrences, or from the prejudice of the mechanical superintendants of the colonial dock-yards, or really, as some have pretended, from an absolute unfitness of the substance in question, the views of the gallant admiral have been invariably thwarted, or his exertions rendered altogether fruitless.

Its importance in this view.

Not fairly tried.

I was at Antigua in 1809, when a transport arrived laden with this pitch for the use of the dock yard at English Harbour: it had evidently been hastily collected with little care or zeal from the beach, and was of course much contaminated with sand and other foreign substances. The best way would probably be to have it properly prepared on the spot, and brought to the state in which it may be serviceable, previously to its exportation. I have frequently seen it used to pay the bottoms of small vessels, for which it is particularly well adapted, as it preserves them from the numerous tribe of worms so abundant in tropical countries†.

A preservative against worms.

* This island contains also a great quantity of valuable timber, and several plants which yield excellent hemp.

† The different kinds of bitumen have always been found particularly obnoxious to the class of insects; there can be little doubt but that they formed ingredients in the Egyptian compound for embalming bodies, and the Arabians are said to avail themselves of them in preserving the trappings of their horses. Vide Jameson's Mineralogy.

There

There seems indeed no reason why it should not, when duly prepared and attenuated, be applicable to all the purposes of the petroleum of Zante, a well known article of commerce in the Adriatic, or that of the district in Burmah, where 400000 hogsheads are said to be collected annually*.

Bitumen thrown up in the neighbouring sea.

It is observed by captain Mallet, in his Short Topographical Sketch of the Island, that "near Cape la Brea (la "Braye) a little to the south-west, is a gulf or vortex, "which in stormy weather gushes out, raising the water "five or six feet, and covers the surface for a considerable "distance with petroleum or tar;" and he adds, that "on "the east coast, in the Bay of Mayaro, there is another "gulf or vortex, similar to the former, which in the "months of March and June produces a detonation like "thunder, having some flame with a thick black smoke, "which vanishes away immediately; in about twenty-four "hours afterward is found along the shore of the bay a "quantity of bitumen or pitch, about three or four inches "thick, which is employed with success". Captain Mallet likewise quotes Gumilla, as stating in his Description of the Orinoco, that about seventy years ago, "a spot of land on "the western coast of this island, near half way between "the capital and Indian village, sunk suddenly, and was "immediately replaced by a small lake of pitch, to the "great terrour of the inhabitants".

Land swallowed up in a pitch lake.

Probably a subterraneous reservoir of it.

I have had no opportunity of ascertaining personally whether these statements are accurate, though sufficiently probable from what is known to occur in other parts of the world; but I have been informed by several persons, that the sea in the neighbourhood of la Braye is occasionally covered with a fluid bitumen, and in the south-eastern part of the island there is certainly a similar collection of this bitumen, though of less extent, and many such detached spots of it are to be met with in the woods: it is even said, that an evident line of communication may thus be traced between the two great receptacles. There is every probability, that in all these cases the pitch was originally fluid,

* Vide Aikin's Dictionary of Chemistry, quoted from Captain Cox in the Asiatic Researches.

an

and has since become inspissated by exposure to the air, as happens in the Dead Sea and other parts of the east.

Geological inquiries difficult in this country.

It is for geologists to explain the origin of this singular phenomenon, and each sect will doubtless give a solution of the difficulty according to its peculiar tenets. To frame any very satisfactory hypothesis on the subject, would require a more exact investigation of the neighbouring country, and particularly to the southward and eastward, which I had not an opportunity of visiting. And it must be remembered, that geological inquiries are not conducted here with that facility which they are in some other parts of the world; the soil is almost universally covered with the thickest and most luxuriant vegetation, and the stranger is soon exhausted and overcome by the scorching rays of a vertical sun. Immediately to the southward, the face of the country, as seen from la Braye, is a good deal broken and rugged, which Mr. Anderson attributes to some convulsion of nature from subterranean fires, in which idea he is confirmed by having found in the neighbouring woods several hot springs. He is indeed of opinion, that this tract has experienced the effects of the volcanic power, which, as he supposes, elevated the great mountains on the main and northern side of the island*. The production of all bituminous substances has certainly with plausibility been attributed to the action of subterranean fires on beds of coal, being separated in a similar manner as when effected by artificial heat, and thus they may be traced through the various transformations of vegetable matter. I was accordingly particular in my inquiries with regard to the existence of beds of coal, but could not learn that there was any certain trace of this substance in the island; and though it may exist at a great depth, I saw no strata that indicate it. A friend indeed gave me specimens of a kind of bituminous shale mixed with sand, which he brought from Point Cedar, about twenty miles distant; and I find Mr. Anderson speaks of the soil near the Pitch-lake containing burnt cinders, but I imagine he may have taken for them the small fragments of the bitumen itself.

Hot springs in the neighbouring woods.

No coal known to exist here.

An examination of this tract of country could not fail, I

* Vide 79th vol. Philos. Trans.; or Ann. Register for 1789.

think,

think, to be highly gratifying to those who embrace the Huttonian theory of the Earth, for they might behold the numerous branches of one of the largest rivers of the world (the Orinoco) bringing down so amazing a quantity of earthy particles as to discolour the sea in a most remarkable manner for many leagues distant*; they might see these earthy particles deposited by the influence of powerful currents on the shores of the gulf of Paria, and particularly on the western side of the island of Trinidad; they might there find vast collections of bituminous substances, beds of porcelain jasper, and such other bodies, as may readily be supposed to arise from the modified action of heat on such vegetable and earthy materials as the waters are known actually to deposit. They would further perceive no very vague traces of subterranean fire, by which these changes may have been effected, and the whole tract elevated above the ordinary level of the general loose soil of the country, as for instance, hot springs, the vortices above mentioned, the frequent occurrence of earthquakes, and two semivolcanic mounds at Point Icaque, which, though not very near, throw light on the general character of the country. Without pledging myself to any par-

Huttonian theory.

* No scene can be more magnificent than that presented on a near approach to the north-western coast of Trinidad. The sea is not only changed from a light green to a deep brown colour, but has in an extraordinary degree, that rippling, confused, and whirling motion, which arises from the violence of contending currents, and which prevail here in so remarkable a manner, particularly at those seasons when the Orinoco is swollen by periodical rains, that vessels are not unfrequently several days or weeks in stemming them, or perhaps are irresistibly borne before them far out of their destined track. The dark verdure of lofty mountains, covered with impenetrable woods to the very summits, whence, in the most humid of climates, torrents impetuously rush through deep ravines to the sea; three narrow passages into the gulf of Paria, between rugged mountains of brown micaceous schist, on the caverous sides of which the eddying surge dashes with fury, and where a vessel must necessarily be for some time embayed, with a depth of water scarcely to be fathomed by the lead, present altogether a scene which may well be conceived to have impressed the mind of the navigator who first beheld it with considerable surprise and awe. Columbus made this land in his third voyage, and gave it the name of the *Bocas del Drago*. From the wonderful discoloration and turbidity of the water, he sagaciously concluded, that a very large river was near, and consequently a great continent.

Magnificent scene.

Inferences of Columbus.

ticular

ticular system of geology, I confess an explanation similar to this appears to me sufficiently probable, and consonant with the known phenomena of nature. A vast river, like the Orinoco, must for ages have rolled down great quantities of woody and vegetable bodies, which from certain causes, as the influence of currents and eddies, may have been arrested and accumulated in particular places; they may there have undergone those transformations and chemical changes, which various vegetable substances similarly situate have been proved to suffer in other parts of the world. An accidental fire, such as is known frequently to occur in the bowels of the Earth, may then have operated in separating and driving off the newly formed bitumen more or less combined with siliceous and argillaceous earths, which forcing its way through the surface, and afterward becoming inspissated by exposure to the air, may have occasioned such scenes as I have ventured to describe. The only other country accurately resembling this part of Trinidad of which I recollect to have read, is that which borders on the gulf of Taman in Crim Tartary: from the representation of travellers, springs of naphtha and petroleum equally abound, and they describe volcanic mounds precisely similar to those of Point Icaque. Pallas's explanation of their origin seems to me very satisfactory, and I think it not improbable, that the River Don and Sea of Azof may have acted the same part in producing these appearances in the one case, as the Orinoco and gulf of Paria appear to have done in the other*. It may be supposed that the destruction of a forest, or perhaps even a great Savanna on the spot, would be a more obvious mode of accounting for this singular phenomenon; but, as I shall immediately state, all this part of the island is of recent alluvial formation, and the land all along this coast is daily receiving a considerable accession from the surrounding water. The Pitch-lake with the circumjacent tract, being now on the margin of the sea, must in like manner have had an origin of no very distant date; besides, according to the above representation of Capt. Mallet, and which has been frequently corroborated, a fluid bitumen oozes up and rises to the sur-

Similar country in Tatary.

* Vide Universal Mag. for Feb. 1808, Mrs. Guthrie's Tour in the Tauride, or Voyages de Pallas.

face

face of the water on both sides of the island, not where the sea has encroached on and overwhelmed the ready-formed land, but where it is obviously in a very rapid manner depositing and forming a new soil.

Mr. Hatchett's supposition.

From a consideration of the great hardness, the specific gravity, and the general external characters of the specimens submitted a few years ago to the examination of Mr. Hatchett, that gentleman was led to suppose, that a considerable part of the aggregate mass at Trinidad was not pure mineral pitch or asphaltum, but rather a porous stone of the argillaceous genus much impregnated with bitumen.

Specimens analysed by him.

Two specimens of the more compact and earthy sort, analysed by Mr. Hatchett, yielded about 32 and 36 per cent of pure bitumen: the residuum in the crucible consisted of a spongy, friable, and ochraceous stone; and 100 parts of it afforded, as far as could be determined by a single trial, of silica 60, alumine 10, oxide of iron 10, carbonaceous matter by estimation 11; not the smallest traces of lime could be discovered, so that the substance has no similarity to the bituminous limestones which have been noticed in different parts of the world*. I have already remarked, that this mineral production differs considerably in different places. The specimens examined by Mr. Hatchett by no means correspond in character with the great mass of the lake, which, in most cases, would doubtless be found to be infinitely more free from combination with earthy substances; though from the mode of origin which I have assigned to it, this intermixture may be regarded as more or less unavoidable. The analysis of the stone after the separation of the bitumen, as Mr. Hatchett very correctly observes, accords with the prevalent soil of the country; and I may add, with the soil daily deposited by the gulf, and with the composition of the porcelain jasper, in immediate contact with the bituminous mass.

Geology of the island.

All the country which I have visited in Trinidad, is either decidedly primitive or alluvial. The great northern range of mountains which runs from east to west, and is connected with the highlands of Paria on the continent by the

* Vide Linnean Trans. vol. 8.

islands

islands at the Bocas, consists of gneiss, of mica slate containing great masses of quartz, and in many places approaching so much to the nature of talc, as to render the soil quite unctuous by its decomposition, and of compact bluish gray limestone, with frequent veins of white crystallized carbonate of lime. From the foot of these mountains for many leagues to the southward there is little else than a thick, fertile, argillaceous soil, without a stone or a single pebble. This tract of land. which is low and perfectly level, is evidently formed by the *detritus* of the mountains, and by the copious tribute of the waters of the Orinoco, which, being deposited by the influence of currents, gradually accumulates, and in a climate where vegetation is astonishingly rapid, is speedily covered with the mangrove and other woods. It is accordingly observed, that the leeward side of the island constantly encroaches on the gulf, and marine shells are frequently found on the land at a considerable distance from the sea. This is the character of Naparima and the greater part of the country I saw along the coast to la Braye. It is not only in forming and extending the coast of Trinidad, that the Orinoco exerts its powerful agency; cooperating with its mighty sister flood, the Amazons, it has manifestly formed all that line of coast and vast extent of country, included between the extreme branches of each river. To use the language of a writer in the Philosophical Transactions of Edinburgh, "If you cast your eye upon the map you will observe, "from Cayenne to the bottom of the gulf of Paria, this "immense tract of swamp formed by the sediment of these "rivers, and a similar tract of shallow muddy coast, which "their continued operation will one day elevate. The "sediment of the Amazons is carried down thus to leeward "(the westward) by the constant currents which set along "from the southward and the coast of Brazil. That of the "Orinoco is detained and allowed to settle near its "mouths by the opposite island of Trinidad, and still more "by the mountains on the main, which are only separated "from that island by the Bocas del Drago. The coast of "Guinea has remained, as it were, the great eddy or resting "place for the washings of great part of South America

Land formed by the Oronoko and the river of Amazons.

"for

"for ages; and its own comparatively small streams have "but modified here and there the grand deposit*."

Having been amply gratified with our visit to this singular place, which to the usual magnificence of the West Indian landscape unites the striking peculiarity of the local scene, we reembarked in our vessel, and stood along the coast on our return. On the way we landed, and visited the plantations of several gentlemen, who received us with hospitality, and made us more fully acquainted with the state of this island: a colony which may with truth be described as fortunate in its situation, fertile in its soil, and rich beyond measure in the productions of nature; presenting, in short, by a rare combination, all which can gratify the curiosity of the naturalist, or the cupidity of the planter; restrained in the developement of its astonishing resources, only by the inadequacy of population, the tedious and ill-defined forms of Spanish justice, and the severe, though we hope transient, pressure of the times. Political view of the island of Trnidad.

VII.

Chemical Experiments on Indigo: *by* M. Chevreul†.

Mr. Vauquelin having requested me to examine the cause of that purple smoke, which arises from indigo exposed to heat, I made some experiments for the purpose, of which the following are the results. Examination of the purple smoke from indigo.

Sect. I. On distilling indigo with a gentle heat, the products were: 1, an ammoniacal water: 2, sulphur, united probably with oily hidrogen: 3, a thick oil of a brown colour, containing carbonate and acetate of ammonia: 4, prussiate and hidroguretted sulphuret of ammonia: 5, a purple Action of heat on indigo.

* Vide Mr. Lochead's Obser. on the Nat. His. of Guiana, Edin. Trans. vol. 4. See Journal, 4to series, vol. II, p. 352.

† Journ. de Phys. vol LXV, p. 309. Abridged from the paper read to the Institute, july the 13th, 1807. A fuller account is inserted in the Ann. de Chim. vol. LXVI, p. 5, of which the translator has occasionally availed himself.

purple matter crystallized in small silky tufts at the summit of the retort: 6, a very bulky nitrogenous coal, yielding a prussiate when calcined with potash: 7, some gasses, which I did not examine.

Best mode of obtaining the purple matter.

The purple matter being the principal object of my research, it was necessary to have recourse to some other mode of obtaining it in a state of purity, for that I obtained by distillation was contaminated with the oil, which arose with it. The process that succeeded best with me was heating in a platina or silver crucible, surrounded by a charcoal fire, 5 dec. [7·7 grs] of indigo in fine powder; when the purple matter crystallized in needles in the middle of the crucible. It is necessary that the crucible be kept well closed during the process, and also for some time after it is removed from the furnace, otherwise the indigo would take fire.

I shall describe below the properties of this sublimed matter, which had not wholly escaped the observation of Bergman; merely observing here, that it is the indigo separated from all those matters with which it is combined in what is sold by this name. At present I shall proceed to examine the nature of these substances, and the methods of separating them.

Analysis of indigo in the humid way. Action of water on it.

Sect. II. *Art.* I. Indigo finely powdered was infused for twelve hours in water heated to 90° or 100° F., in a closed glass vessel. The decanted liquor retaining some indigo in suspension, it was filtered; and the indigo was exhausted by repeated infusion and decoction.

Disoxigenated indigo.

a. These liquors being united and distilled yielded an odoriferous water, a little ammoniacal; and I suspect it contained also sulphur. Mean time a greenish powder was precipitated from it, which assumed a blue colour from contact with the air. This substance exhibited all the characters of indigo, whence I infer, that part of the indigo in that of the shops is disoxigenated, and dissolves in water by means of the ammonia.

Green matter.

b. Long after the separation of the disoxigenated indigo, a flocculent precipitate appeared of a peculiar substance, which I shall call *green matter*; and which had the following properties. It is very little soluble in water, unless by the intermedium of an alkali. It then assumes a reddish colour,

colour, which acids change to a green by saturating the alkali. When the solutions are concentrated, the green matter falls down in green flocks. Alcohol dissolves this precipitate, and forms a red tincture; but this, when spread out thin, or mixed with water, appears green, as it does when viewed on its surface.

c. Alcohol being added to the concentrated liquor *b*, from which the green matter had been precipitated, separated a substance, the taste of which was slightly bitter and astringent, and which burnt on the coals, diffusing a smell of empyreumatic vinegar. The alcohol acquired a reddish colour, owing to the combination of green matter with ammonia.

Thus the substances separated from the indigo by water were, 1, ammonia: 2, indigo at a minimum of oxidation: 3, a green matter: 4, a slightly bitter and astringent matter, of a yellowish brown colour. Of these the 2d and 3d are held in solution by the ammonia.

100 parts of indigo lost 12 by treatment with water.

Art. II. From the indigo exhausted by water alcohol took up, 1, some green matter: 2, a matter that I call red resin: 3, indigo at a maximum of oxidation. Action of alcohol.

The insolubility of the green matter in the treatment with water (*Art.* I.) I ascribe to the want of a sufficient quantity of ammonia to dissolve it entirely, and the affinity of the red matter for it.

The principal difference between the red resin and the green matter is, that the latter is rendered red by alkalis, and that this compound becomes green by the addition of an acid; while the colour of the former is not changed either by acids or alkalis, only acids produce with it a red flocculent precipitate.

In acting twice on the indigo alcohol took up 26 parts from the 88 left by the water. I suffered the alcohol to act on it no longer, when it began to acquire a violet tint.

Art. III. Muriatic acid dissolved 10 parts; 2 of which were iron mixed with alumine, 2 carbonate of lime, and 6 probably red matter, that was dissolved in the acid after being decomposed. Action of muriatic acid.

The preceding experiment having shown, that the indigo Farther action of alcohol.
was

was not completely divested of foreign colouring matter, I treated it again with alcohol, till this liquid became blue. By this treatment it lost 4 parts of red resin, mixed with a little indigo.

In these different processes the indigo lost 0·52 of foreign matter, which reduced it to 0·48, from which 0·03 more must be deducted for the silex it still contains.

Indigoes differ. Every sort of indigo does not yield the same results on analysing as that of Guatimala, on which I operated. In most the green matter changed to a fawn colour; it became very red on the addition of alkalis; but acids did not render this compound green. One specimen, in pretty thick square cakes, of a black blue colour, yielded me no indigo at a minimum. Its ashes contained more iron than that of Guatimala, and also magnesia. Some indigo, which I was informed came from Bengal, yielded me a twentieth of indigo at a minimum; and its ashes contained a little sulphate of lime. In some indigoes I found traces of phosphate of lime.

Green matter variable. It is not very common to find the green matter in full possession of its properties: sometimes yellow extractive matter is so predominant, that it is difficult to detect it; and sometimes no vestige of it is to be found. In general I remarked, that those indigoes, which contained most ammonia, contained also more indigo at a minimum, and more green matter, than others. The indigo of Java afforded me the last in its greatest purity.

Colouring matter variously modified. I consider the colouring matters accompanying indigo as originating from the same substance variously modified.

Purple smoke. *Sect.* III. The source of the purple smoke was now easily detected. On heating successively the green matter, extract, and gum, extracted by water, and the red resin extracted by alcohol, no purple smoke was perceivable. But trying the same experiment on the indigo separated by water, on that separated by alcohol, and lastly on that treated successively by water, alcohol, and muriatic acid, a fine purple smoke arose, much deeper coloured than that produced by an equal quantity of indigo not purified.

the pure indigo sublimed. This smoke is not the result of a decomposition of the indigo by heat: for we found by experiment, that it was this

this colouring matter itself volatilized; and that the substance crystallized in silky tufts, obtained by distilling indigo, is indigo in a state of purity. These crystals dissolve in concentrated sulphuric acid, imparting to it a fine blue colour; and are volatilized anew in a purple smoke, when thrown on a hot body.

Indigo, therefore, is volatile, and capable of crystallization; and may be purified either in the dry or in the wet way. The indigo obtained in both ways is perfectly similar, except that the latter always retains some earthy matter: and it is remarkable, that the indigo purified in the wet way is not so blue as it was before, and has a perceptible violet tinge; while indigo not purified, if placed by its side, appears of a dull blue. Purified indigo.

When pure indigo is thrown into concentrated sulphuric acid, it first produces a yellow, which afterward becomes green, and at length of a fine blue. In this process the indigo undergoes some change of composition, that merits examination. This is shown, by its being soluble in a number of menstruums, after it has been precipitated from this solution, which before had no action on it: and, which is more strange, by its no longer producing the purple smoke, at least in the same circumstances, and appearing to have lost its volatility. Action of sulphuric acid on it.

Hot alcohol dissolves a small portion of indigo, which gives it a fine blue colour; but as it cools the colouring matter falls down, and after some time scarcely any is retained in solution. If however the indigo contain a certain quantity of the red resin, the solution will remain coloured for some months. Action of alcohol.

From the facts adduced it follows:

1, That pure indigo is purple: General properties of indigo.

2, That it is volatilized in the form of a purple smoke, crystallizable in needles of the same colour:

3, This volatization of a highly carbonated substance is remarkable, as it demonstrates, that the volatility of compounds does not depend simply on the volatility of their elements, but also on the affinity, with which the most dilutable are united to the most fixed:

4, Indigo is a little soluble in alcohol.

A very

Indigo disoxidated by sulphuretted hidrogen.

A very interesting observation, for which we are indebted to Mr. Vauquelin, is the disoxidation of indigo by sulphuretted hidrogen. This experiment proves two curious facts: 1st, that in this substance either the whole or at least part of the oxigen exists in some sort separate from the other principles, since it may be taken away, and restored at pleasure by allowing the sulphuretted hidrogen to evaporate in the open air, without destroying the nature of the colouring matter. In this circumstance indigo has a resemblance to the metals. 2dly, that carbon has no concern in the colouring of indigo, since this is deprived of colour in circumstances in which it contains most carbon.

VIII.

On the Action of Muriatic Acid on Sugar, and the Nature of its Principles: In a Letter from John Nowell, *Esq.*

To W. NICHOLSON, Esq.

SIR,

Nitric acid decomposed on sugar by yielding up one of its elements, the oxigen, might furnish a clew for investigating its composition.

IT is well known, that the nitric acid becomes decomposed with sugar under certain circumstances, and forms a vegetable acid (the oxalic) by yielding to the sugar one of its elements. If the composition of the nitric acid was not known, this property evidently would furnish a clew to guide us in the investigation of its elementary principles. Some time ago I was struck with the same idea with respect to the muriatic acid; and, as its action on sugar had not been observed with attention, I set about making experiments on the subject, with a view, if not to change the muriatic acid into a new substance, at least to satisfy myself of the particulars of its action.

The same may be said respecting muriatic acid.

Dr Priestley partially examined the effects produced on sugar by muriatic gas.

I was aware, that Dr. Priestley had observed when muriatic gas was passed through a solution of sugar it gradually acquired a brown colour and strong smell; but on passing a current of this gas through a moderately strong solution, I was convinced of the extreme slowness of the process. Besides,

Besides I did not observe the effects as he describes them, till heating the mixture, when it grew black, and carbon became deposited.

On account of the slowness of the process I substituted the weak liquid muriatic acid of the specific gravity of 1·050 or 1·080 instead of the gas, having first satisfied myself by experiments of the analogy of the results*. In some former experiments on the action of the oximuriatic gas on sugar assisted by heat, I had obtained the same results, and drawn the same conclusions, as Mr. Chenevix, though his results and conclusions were at that time unknown to me, it being only lately that I saw them in the last edition of Dr. Thomson's System of Chemistry. Mr. Chenevix thinks, that the oxigen of the oximuriatic acid goes to the formation of the malic acid, which is produced during the action; but as the experiments detailed in this paper will prove, that the muriatic acid acts with facility on sugar, we can scarcely doubt, that, after all the oxigen has been given to the elements of sugar from the oximuriatic acid, the muriatic acid acts on the remaining sugar, being thereby partially decomposed.

The liquid acid substituted instead of the gas

Oximuriatic acid assisted by caloric has considerable action on sugar:

it forms the malic acid.

Vauquelin does not mention the formation of the malic acid when sugar is acted upon by the oximuriatic gas, but says, "that the solution possessed the properties of caramel or partially burnt sugar." I have often been at a loss what substance to ascribe this French name to, whether to a new product formed during the decomposition of sugar by heat, or to the fumes of the pyromucous or acetic acid, which are given off plentifully. But, if by caramel is meant partially burnt sugar, we may altogether discard this name from our chemical systems, and substitute the old name molasses instead of it†. Under certain circumstances that

Mr Vauquelin overlooked the malic acid when he examined the action of the oximuriatic acid on sugar.

* My reason for substituting the weak acid instead of the strong was, that, as the strong acid occupies considerably less bulk, no large quantity of sugar would be dissolved; for, when the sugar is added in large quantity, the acid becomes diffused through its pores by capillary attraction. There can be no doubt however, that the action of both is perfectly analogous.

† "Caramel. Saccharum percoctum. Drogue que les apoticaires préparent pour le rhume, qui consiste particuliérement en du sucre fort cuit." Encyc. Franç. Lat. et Ang. Lond. 1761. C.

this

this substance is present is sometimes the case, though we do not raise the heat high; but that the malic acid exists in abundance there cannot be the least doubt, notwithstanding the opinion of such an able chemist as Mr. Vauquelin.

The acid was pure.

The muriatic acid used in all the following experiments was pure. It gave no indication of any foreign ingredients by the usual reagents.

Muriatic acid at a low temperature dissolves sugar without decomposing it.

SECT. I. 1st. 50 grs of muriatic acid of the spec. grav. 1·050 were added to 50 grs of loaf sugar at the temperature 45° Far. The sugar dissolved without effervescence. The taste of the solution was acid, though slightly saccharine. The original stiffness of the acid was somewhat increased, and its colour changed to that of a yellowish brown. Saturated with a solution of subcarbonate of soda, and evaporated at 212°, it gradually acquired the consistence of a sirup; and very pungent white vapours were given off, which condensed on the lid that covered the capsule. From their taste and smell they appeared to be the pyromucous acid. If the 50 grs above had been saturated with soda, the muriate would have weighed 14 grs. 14 grs of muriate of soda were mixed with 50 grs of sugar dissolved in water, and submitted in every respect to the same operation as the solution of sugar in muriatic acid; when exactly the same phenomena presented themselves as in the former case, viz. the mixture acquired a sirupy consistence, and towards the close of the evaporation emitted acetic fumes. Hence it appears, that this change takes place without free muriatic acid being present; of course this acid had no share in the decomposition. This change took place exactly the same when a solution of sugar was evaporated rapidly; from which I infer, that cold muriatic acid has no action on sugar, except as a solvent. Whether it be the water contained in abundance in the dilute acid, which dissolves the sugar, or in some measure the acid itself, it would not be very easy to decide.

Muriatic acid assisted by heat has considerable action on sugar.

If, instead of saturating the solution of sugar in muriatic acid with soda, we apply a slight heat for some time, the mixture becomes black, and carbon precipitates. To obtain all the products of this apparent decomposition, I made use of the following apparatus. A small retort was

Description of

joined

joined to a receiver with two necks; into one neck the beak of the retort was inserted, into the other a glass tube, which terminated in a glass air holder filled with water. The tubes were fitted through corks into their respective necks, and luted perfectly air tight with bees wax, or with resin. By this apparatus I was enabled to ascertain whether any gas, except the air of the vessels, came over during the application of heat, at the same time that the air holder had not the inconvenience that a common plain tube, terminating under the pneumatic shelf, would have had of admitting the water of the trough into the receiver, when impelled to it by the sudden condensation of the aqueous vapour in the retort. I shall now proceed to give the experiments as the facts presented themselves during the inquiry, being persuaded that this method is the most accurate, as well as the most concise.

the apparatus made use of.

SECT. II. 1st. 100 grs of muriatic acid, spec. grav. 1·080, were mixed with 100 grs of loaf sugar. A solution of the sugar was effected, accompanied by the emission of a slight pungent vapour of muriatic acid. This solution being introduced into the retort, and joined and luted to the auxiliary apparatus before described, the heat of 180° F. was applied by means of a water bath for half an hour. After about ten minutes had elapsed, abundance of carbon became deposited, and adhered to the bottom and sides of the retort firmly; till finally the solution became apparently solid from the copious deposition of carbon. During this change, not the least quantity of gas came over, except the air of the vessels, which returned again on suffering the apparatus to cool.

Heat applied to the solution of sugar in dilute muriatic acid precipitates carbon.

No gas came over in the process above.

2d. The liquid found in the receiver weighed 7 grs; of course a large portion must have adhered to the carbonaceous matter in the retort. But in subsequent experiments, on using a retort that exposed a larger surface of the liquid to evaporation, I have known it amount to 70 or 80 grs, though only exposed to heat the same time. Still, whatever may be the quantity which comes over, it always consists of two acids, the muriatic and pyromucous, or rather the acetic a little modified. If to this liquid we add carbonate of lead, an effervescence is the result, mu-

The liquid distilled into the receiver consisted of pyromucous acid and the muriatic.

riate of lead falls down; and by employing a close filter we may separate the insoluble muriate from the acetate, which passes through the filter. By saturation with soda the oxide is precipitated, and by evaporating the mother water we obtain the acetate, or at least the apparent acetate, dissoluble in rectified alcohol. The acid, which holds the lead in solution, appears more susceptible of being driven off by heat than the acid of the common acetate; for I have several times observed, that, when a solution of lead in it is concentrated by evaporation, a pungent smell is given off, and a yellow oxide is precipitated.

A partial solution of the substance in the retort effected.

3d. The residuum in the retort was detached by 1000 grs of water, added in quantities of 100 grs at a time, and employing some agitation. A partial solution of this substance was effected.

Carbon not pure.

4th. The substance insoluble being separated by the filter, it appeared to be carbon, though when heated it gave off gaseous inflammable matter. I have seen some sorts of impure charcoal do the same, under similar circumstances*.

The solution possessed the properties generally of muriatic and malic acid.

5th. The solution, which passed through the filter, was of the colour of red wine: its taste was acid, and it reddened vegetable blues. The various reagents generally adopted by our most eminent chemists did not indicate the presence of any of the following acids, viz. the gallic, oxalic, tartaric, and citric; neither did the benzoic, suberic, succinic, or camphoric exist in it. The only products, beside muriatic acid and a little undecomposed sugar, were a large quantity of malic and a trace of the acetic acid. To a known quantity carbonate of lead was added to saturation; the malate and muriate of lead were separated by the filter, and the acetate passed through. The same evidence of the presence of this acid was obtained as in sect. II. The substance left on the filter was of a brown colour. After being

A small portion of the acetic likewise detected, or rather the pyromucous.

* The whole weight of this substance when perfectly dry would be about 36 grs. 10 grs, being heated red hot for some time, lost in weight 4 grs. Therefore 36 grs would lose 14·4; so that, if we could take into account all the carbonate in the product drawn off by heat, the quantity would probably differ little from the statement of Lavoisier, viz. in 100 parts 28 grs.

being well washed, a small quantity of dilute sulphuric acid was poured upon it. The mixed sulphate of lead and muriate were separated by the filter. What passed through possessed the original brown colour, and in part the acid taste; and had the properties of the malic acid, though it was evidently mixed with a small quantity of the muriatic. It is very difficult to separate these two acids from each other, without resolving the malic into its ultimate elements; the reagents being acted upon by each somewhat alike. In the above case the acetate of lead precipitated both the acids; and the sulphuric acid acted not only upon the malate, but also on the muriate: consequently instead of finding the malic acid singly, a mixture of the malic acid and muriatic were found. A fact which at first appeared somewhat puzzling to me was, that, on introducing a quantity of this fluid into a retort, and gently distilling, a large quantity of acid was found in the receiver; which, examined nearly by the method just mentioned, appeared to be of the nature of the acetous. If we apply heat a long time to the carbonaceous matter, which is plentifully deposited during the distillation, so as to drive off all the adhering acid; on macerating the dry mass in water we do not find a solution of malic acid, but sometimes, under certain circumstances, something of the remains of sugar*. This curious change is owing to the presence of muriatic acid, as the following comparative experiment will prove.

Curious fact: some of the liquid being distilled deposited carbon, and yielded pyromucous acid as a product.

I prepared some tolerably pure malic acid by bruising the leaves of the sempervivum tectorum (houseleek) along with a little water in an earthen mortar. The juicy mixture thus obtained contained a considerable portion of malate of lime. To remove the lime from the malate, a solution of oxalic acid was added cautiously, and the small excess was removed by lime water. The oxalate of lime was separated by a filter, and the liquid evaporated, till it became sufficiently concentrated. About a drachm of it

Preparation of malic acid from the plant houseleek.

Malic acid distilled by itself does not deposit carbon or

* By returning the acid product into the retort, and distilling successively several times, this substance gradually disappears altogether, and the products are the acetous and muriatic acids and carbon.

was

yield pyromucous acid, but does when distilled with muriatic acid.

was introduced into a very small retort, and gradually distilled to dryness; no carbon became deposited, nor was any acid distilled into the receiver. The dry mass was again dissolved in water, and again distilled along with a few drops of muriatic acid; abundance of carbon now precipitated, and acetous acid was the product found in the receiver along with the muriatic.

Action of the muriatic acid on sugar something analogous to the action of the nitric; some element must be furnished.

As it would be absurd and vague to suppose such decompositions as the above could possibly take place without some new substance being furnished, and as the caloric would have been quite insufficient had not muriatic acid been present, we must of consequence suppose, that this acid is a compound body, capable of furnishing something analogous to that furnished by the nitric acid to sugar in similar situations; for the nitric in fact beside the oxalic forms a portion of malic acid, the quantity of which depends on circumstances. In some cases instead of finding oxalic acid I have found nearly the whole product malic acid, at the same time that something like carbon was deposited*. But if a part of the muriatic acid is furnished, to cause the elements of sugar to be differently arranged, of course it must be decomposed; that is, it must be reduced into its primary elements. The following fact is analogous: when the nitric acid changes the sugar into the oxalic acid, oxigen is furnished, and the other element, the azote, is given off in a combination with a smaller portion of oxigen, in the form of nitrous gas. This analogy would lead us to suppose, that to change sugar into the malic acid, at least some part of the muriatic must disappear, and enter along with the gasseous elements into the composition of the products, viz. the malic and acetous acids; not indeed in the form of muriatic acid, but in the form of some of its primitive elements. But before we can say much more on this subject, we must obtain positive evidence of its partial disappearance, because without such evidence, a nearly similar explanation of the above fact might be given, as that which Mr. Kind gave when he observed the change, that oil of turpentine underwent when acted upon by muriatic

Some part of the muriatic acid must disappear and be decomposed.

* In this case I cannot answer for the purity of the acid. The nitric of commerce sometimes contains muriatic acid.

gas.

gas. But if we obtain such evidence, then it at once follows, that this acid is a compound body; and that its disappearance, when made to act upon sugar, is owing to its ultimate decomposition. To ascertain this important point, after adopting several methods, I was led finally to pitch upon the following as the most susceptible of accuracy. The apparatus made use of for this purpose differed from the former only by a substitution of a tubulated retort for a common one.

SECT. III. 1st. One hundred grains of muriatic acid of the spec. grav. 1·050 were poured upon 50 grs of dry sugar, previously weighed and introduced into the retort. The apparatus was joined, and found to be perfectly air tight. After the sugar was dissolved, heat was applied to the retort, till about 90 grains of liquid were distilled over into the receiver. After the apparatus had become cool by several hours standing, the 90 grs just mentioned were poured back upon the carbonaceous matter in the retort, and again distilled in this manner five times, till finally heat was applied to the retort several hours, to drive off all the adhering acid. Care was taken in all this operation not to disjoin the apparatus, till it had been cool for some time, lest some vapour might rush out, and falsify the results. No extra gas passed over into the air holder, nor had the least sensible quantity of muriatic gas become condensed by its water, for it afforded no muriate with nitrate of silver.

2d. The liquid condensed in the receiver weighed 128 grs. Its colour was a reddish brown: its taste extremely acid: its smell that of aromatic vinegar nearly.

3d. The substance in the retort was tasteless. Water dissolved no part of it, but acquired an acid taste from a number of drops condensed in the neck of the retort*. The whole was thrown upon a filter to separate the carbon, which weighed, after being well washed and dried at 170° or 180° for some time, 18 grs. The liquid, which passed through, weighing 550 grs, gave a precipitate with sulphate of silver weighing 1·375.

* It contained neither a trace of malic acid nor a vestige of undecomposed sugar. The successive distillations having been with the presence of muriatic acid capable of decomposing both.

4th,

Proof that the acid in part disappears by comparing the weight of muriate of silver yielded by the original acid, with that yielded by the same quantity of acid after having decomposed sugar.

4th. Ten grains of the original muriatic acid gave a precipitate with sulphate of silver, which weighed exactly 7 grs after having been dried perfectly on the vapour bath at 170 or 180 degrees. After this rate 100 grs, the quantity used in the experiment above, should yield exactly 70 grains dried at the same heat. 10 grs of the liquid (2d) gave with the same solution of sulphate of silver 4·937 grs dried the same exactly; therefore 128 would have given 63·194 nearly, which, added to the quantity of muriate of silver yielded by the 550 grs of liquid (3d), makes the whole amount of muriate of silver 64·569; which subtracted from 70 grs, the weight that would have been obtained had we operated on the original acid, leaves for deficiency 5·431. According to Dr. Marcet 100 grs of dry muriate of silver contain 19·05 of acid: taking this datum, 5·431 will contain 1·034, which is obviously the loss of real acid. I am at a loss to know, what objections may be brought against this experiment: for my part I can at present see none. The greatest care was taken, that no acid vapour might be lost in the various openings of the apparatus; and I have reason to believe, that not the least escaped, for the weight of the distilled product, which was 128 grs, compared with the few drops of liquid, that remained in the retort, made up along with the carbon the weight of the substances introduced. The muriate of silver in both cases was I think equally dried: both specimens were brought to the greatest state of dryness, by being exposed to exactly the same heat, and particular precaution was taken to bring each to the same state directly before being weighed. It gave me not a little uneasiness to obtain results, that would in any respect militate against the prevailing theory of sir Humphrey Davy. The last experiment I repeated several times with the greatest care, and I always obtained results little differing from the above. From their constant uniformity I cannot conclude less, than that a part of the acid disappears. To explain the rationale of the above fact I had first recourse to the present prevailing theory proposed by sir H. Davy, which supposes the muriatic acid to be compounded of hidrogen and chlorine gas; but from facts directly to be detailed I found it incapable, at least without bordering too much upon hypothesis.

In the experiment (sect. III) the new substances produced during the decomposition were a quantity of the pyromucous acid and water. I endeavoured to ascertain their relative proportions to each other by proceeding on the data of I think Vauquelin, that the pyromucous acid differs only from the acetic in being combined with an oil: though I did not succeed, being persuaded from several facts, that it either differs much from the acetic in composition, or otherwise that errour attends the analysis of the acetic acid by Dr. Higgins. I however saw evidence of the production of water to a considerable amount; and I can entertain but little doubt, that the pyromucous acid consists of oxigen, hidrogen, and carbon, though we do not know its absolute composition. To explain the above facts on the basis of sir H. Davy's theory, we must in the first place suppose, that hidrogen must be furnished to sugar to form the malic acid and the pyromucous, and that the other component part of muriatic acid, the chlorine, must be given off in the gaseous state of oximuriatic gas. But this explanation is insufficient, for the most delicate test that I could apply did not discover a trace of this gas. I am aware however, that a small quantity might adhere along with the muriatic acid insensible to our most delicate tests, as is certainly the case with the ordinary muriatic acid of commerce; but the quantity, which according to sir H. Davy we should have a right to expect, could not from its magnitude have operated in this manner. In the second place it might be supposed, that both these substances were furnished, viz. the chlorine gas and hidrogen: but this supposition would not in the least tally with the known component parts of the water and pyromucous acid, the new products. Sugar is composed of oxigen, hidrogen, and carbon; and the products of the decomposition are composed of the same substances, differing only in the relative proportions of their component parts. Hidrogen or oxigen indeed might have been furnished, but no other substance differing from these was furnished, nor could be furnished without forming a quaternary compound, which we are at present not acquainted with*. The excess of ingredients in this decomposition being only in oxigen and hidrogen, and

The whole products of the last decomposition of sugar were water and pyromucous acid.

Sir H. Davy's theory of the compound nature of the muriatic acid insufficient to explain the rationale of the above facts.

The strictest analogy would lead us naturally to suppose, that the muriatic acid is composed of oxigen and hidrogen.

* Consisting of oxigen, hidrogen, chlorine, and carbon.

as

as no gaseous matter whatever escaped, must we not suppose, that both component parts of the muriatic acid which disappeared entered into the composition of the two products, water and pyromucous acid? If only one entered, the other would be given off; but this was not the case, for no gas whatever, as I have shown before, was produced; of consequence we may I think conclude, that muriatic acid is composed of oxigen and hidrogen.

Reasons for making the oximuriatic acid compound.

Upon strict analogy we cannot conclude less than that the oximuriatic gas or chlorine gas of Davy is a compound. This when heated along with sugar forms malic acid even in more abundance than the muriatic acid does. The malic acid, when submitted to heat capable of decomposing it into its elementary principles, gives us an acid (the pyromucous), water, a large portion of carbonic acid, and some carburetted hidrogen. Hence it must be composed like sugar of oxigen, hidrogen, and carbon: consequently the malic acid is of known composition. If the chlorine gas was simple we could not obtain bodies the composition of which is known, and in which no such principle is found. Instead of obtaining malic acid, which is a ternary combination, we should have obtained of course a quaternary compound, or a direct compound of oxigen, hidrogen, carbon and chlorine; which would have been a body unknown to us, or a new substance. If I can in any degree draw the attention of your more able correspondents to this subject, so as to enlarge more upon it, my sole aim will be fully answered.

Farnley Wood, near Huddersfield, I. N.
June the 10*th*, 1812.

IX.

On the Zig-zag Motion of the Electric Spark. In a Letter from I. A. De Luc, Esq. F. R. S.

To W. NICHOLSON, Esq.

Sir,

Papers in the Journal on electricity.

I have found in your No. 144, two papers, on which I shall take the liberty of communicating to you some remarks: one

one is Art. II, signed J. PHŒNIX, concerning the *zig-zag motion of the electric spark*; and the other Art. XI, by DR. MAYCOCK on *the production of electrical excitement by friction*, which is the continuation of another in your No. 131. These papers concerning *electricity* have strongly excited my attention, as you may suppose from my papers on the same subject in your Journal. But for the present I shall confine myself to the paper signed J. PHŒNIX, on the *zig-zag motion of the electric spark*.

Cause of the zigzag motion of the spark.

The author says (p. 243), "that this subject seems to "have been withheld entirely from public discussion." But he immediately mentions the true explanation of this phenomenon in the following manner. "The only account I "have heard in lectures was, that by its own *rapidity* of "motion it *condensates* the *air* to such a degree, that it has "to move, as it were, from a *solid* to a less *dense medium*; "which seems to me *impossible*." I shall first consider this rejected explanation with respect, not only to its *possibility*, but to its *sufficiency*.

Capable of compressing the air before it, so as to be repelled sideways.

The *electric fluid* moves with a great *velocity*, as we may judge by the sight; and it is such, that we cannot estimate it, comparatively to that of *light*; but it is much *denser*, as we see by the *hole* that a strong *spark* produces in a card which is opposed to its course; it may therefore occasion a sufficient *compression* in the *air* before it, so that at last it is repulsed sidewise.

Example of this in the air.

We have an example of the *repulsion* in the *air* itself. The instrument called *anemometer* shows the *velocity* of the *wind*, because the *air* in motion, finding in it an obstacle, is *condensed* against it, and thus *presses* it forward; but if it finds less *resistance* on one side, it *escapes* and presses the obstacle *sidewise*. The immediate *pressure* of *air* is shown in the ingenious *anemometer* of Dr. LIND, described in the 65th vol. of the *Philos. Transactions*, p. 363. This instrument consists of a glass siphon, having quicksilver in both its branches, open at their extremities, one of which is bent forward at a right angle. When the siphon is held upright, and the opening of the bent branch is turned towards the *wind*, the quicksilver is *depressed* in it, and *ascends* in the other, in proportion to the *velocity* of the *current of air*.

As

Lateral pressure of air on the sails of a ship.

As for the *lateral pressure of air*, when it experiences *less resistance* on one side of its course than on the other, we have an example of this effect in the motions of *ships*; why do they change their course by the different *inclination* of their *sails?* It is because they offer *less resistance* to the motion of the *air*, which thus changes its course; however it *presses sidewise*, so as to put the *ship* in a different motion, which is determined by the rudder. This is an example absolutely analogous, only inverse, of the change of course of the *electric spark*; this compresses the *air*, until finding *less resistance* on one side, it suddenly changes its course.

Erroneous hypothesis.

I come to the author's hypothesis, in which he sets out from this certain fact; "that the *electric fluid* passes in a "more direct line according to the best or the worst *conduct*-"*ing* substances presented to it:" but not being sufficiently conversant with *meteorological* phœnomena, he makes an hypothesis, which will give me the opportunity of showing how necessary is their knowledge in every branch of experimental philosophy, to avoid arbitrary, and even delusive hypotheses. "Our atmosphere," he says, "being a compound "of *oxigen*, &c. presents at once, to the *spark*, flying from "the machine, at least four known *gasses*; all, *I have not* "*the smallest doubt*, differing in their *conducting* power, *were* "*they separately tried*." This therefore remains a mere *hypothesis*, till the *trial* has been made; however he thus continues: "*This point being ascertained*, the phenomenon is "at once accounted for. The fluid flies to the next *best* con-"ducting *gas* from a *worse*, as it would from different por-"tions of matter."

Importance of meteorological phenomena in science.

I hope the author will see now, that he has not *accounted* for this phenomenon. But, Sir, he himself, or others of your readers, will I hope take some interest in a short account of the *meterological* phenomena, which might have prevented his hypothesis, in the first class of which are the following.

Nature of the atmosphere.

I have proved in my work *Idées sur la Météorologie*,—1. That it is an errour to consider the principal mass of the atmosphere as composed of two distinct fluids, or *gasses*, one called *oxigen*, the other *hidrogen*; that *atmospheric air* is a fluid *sui generis*, composed, in each particle, of all the ingredients manifested in its *decompositions*.—2 That *atmospheric air*

air is a transformation of the *aqueous vapour* which constantly ascends in the atmosphere.—3 That *rain* is produced by the *decomposition* of that *air*, which returns to *aqueous vapour*, first in *clouds*, from which, by their condensation, *rain* proceeds.

Those among natural philosophers who have not adopted this system, being however obliged to explain the production of *rain*, have supposed that the *aqueous vapour*, ascending in the atmosphere, accumulates in its upper parts, where it is condensed by the *cold* of that region. But in the first place it has been found by Mr. DE SAUSSURE, and myself, by *hygroscopical* observations, that the more we ascend in the atmosphere, the less of *aqueous vapour* is mixed with the *air*. Besides, from this hypothesis, *rain* should fall only in the *night*, when the atmosphere *cools* after sunset. But the spring of this year has furnished a test to the atmospheric systems. We have had almost incessant *rains*, with great *storms*. Where could that enormous quantity of *water* be contained, if not in the composition of the air itself?—What could have occasioned these tremendous local *storms*, except the decomposition of *air* in certain *extents*, toward which the other *air* was rushing? Different hypothesis of rain inconsistent with facts.

However this analysis of the constitution of the atmosphere is not necessary to show how groundless the author's hypothesis is; for it is a known fact, that if such distinct *gasses* as *oxigen* and *hidrogen* exist in its mass, they are no where *separated* in the whole of its extent, from the plain to the top of the highest mountains: consequently the *electric spark* can no where be attracted on one side more than another, even were it proved that these *fluids* possess different *conducting* faculties. Therefore there remains only the explanation which the author rejects, because he was not informed of these facts. Another objection to the author's hypothesis.

There is a phenomenon, which shows to the sight the manner in which some *fluids*, distinct from *atmospheric air*, ascending in the atmosphere, follow their course; I mean what is called *falling stars*, when they follow a long track. This is a *phosphoric* fluid, ascending from some spot of the surface of the Earth. It is invisible in its ascent, because there is some circumstance required to make it *phosphorescent*, Falling stars.

cent, by decomposition; but when this happens, the *light* disengaged makes it visible the whole way, and this is in a *straight line*. The small *falling stars* are composed of the same *fluid*, but it has been disturbed in its ascent by the agitation of the air; its *streams* have been divided, and their *direction* changed.

If the author has any objection to the whole, or to some part of this answer to his system, I shall be glad to receive it through your Journal; but he will find, I think, that it involves many more objects of *meteorology* than he was aware of; as this is connected with most part of natural philosophy, I remain, SIR,

your most obedient servant,

J. E. DE LUC.

Windsor, June the 18*th*, 1812.

X.

Remarks on an artificial stony Substance: *by* F. R. CURAUDAU*.

Solidification of water.

A Remarkable example of the high degree of solidification that water can acquire in certain combinations is exhibited by the artificial stones, which form the subject of the present remark.

Composition of an artificial stone.

These stones, more than half the weight of which is water, consist also of sulphuric acid and baked clay reduced to powder, in the proportion of one part of the former and two of the latter. The simple mixture of these three substances affords only a solution of sulphate of alumine: but, if their mutual action be promoted, heat is soon produced, and its evolution is sometimes so considerable, that the matter seems incandescent.

Action of a large quantity of materials.

If the quantity of materials amount to 25 or 30 hundred weight, this beautiful phenomenon lasts above an hour. But, what is particularly remarkable, if the matter come to want water at the moment when the mutual action of the

* Journ. de Phys. vol. LXVIII, p. 409.

substances

substances on each other is most energetic, the mass, though still fluid, acquires suddenly a great degree of solidity; the heat is even increased in its intensity; and the matter afterward passes almost wholly to a state of insolubility. The latter property, acquired by a mixture intended to produce very soluble salts, proves, that the penetration of the earth by the water and acid must have been very great, since the whole mass forms only a stony compound.

A stone similar in appearance, but not insoluble.

The stones to which I here allude, though having in appearance all the properties of those I have just described, have not the quality of being insoluble. On the contrary I prevent their passing to this state, as then I could not make use of them. But as this compound has all the external characters of the hardest stones, except that it is not insoluble, I conceived it would not be uninteresting to see an artificial stony substance, which some peculiar properties might render useful. For instance, as it may be softened by a heat superior to that of boiling water, might it not be employed with much advantage for fastening iron or wood in stone, casting statues, moulding vases, and many other purposes, that experience would point out? It is true that substances formed of this stony paste must not be exposed to wet.

Applicable to different uses.

Cause of volcanic eruptions,

Another consideration, that has led me to suppose this new stony compound would not be viewed with indifference, is, that the theory of its formation, and its analogy with the stones of solfaterras, render it unnecessary for us to have recourse to the hypothesis of subterranean fires kept up by combustible matters, to explain the eruptions of volcanoes.

In fact, since water alone, by passing instantaneously from the liquid to the solid state, can give rise to the evolution of so very considerable a degree of heat, may it not be the immediate cause of volcanic eruptions? Is it not likewise the slow and gradual passage of water to the solid state, that produces the heat kept up at great depths in the interior of the globe? Lastly, is not the heat developed in animal and vegetable organization equally owing to water?

interior heat of the Earth,

and organic bodies.

SCIENTIFIC

SCIENTIFIC NEWS.

Society for the Encouragement of Arts, &c.

Society of Arts, &c. Premiums for planting forest trees.

In the year 1808, the gold medal of the Society of Arts &c. was adjudged to Dr. Bain, of Curzon-street, for planting 338199 forest trees, at Heffleton, in Dorsetshire, in 1804 and 1805. These were part of more than eight hundred thousand, that he had planted from 1798 to that time on a heath valued to the tenant at 1s. an acre per annum, a poor gravelly soil, on a situation rather elevated, and much exposed to the winds from the seacoast. Thus encouraged, and the trees for the most part thriving well, the Dr. has pursued his exertions, adding near three hundred thousand trees more to his plantations, on ground not adapted to the purposes of husbandry. The trees are chiefly larch, pinaster, and Scotch fir; the last in much the largest proportion. The luxuriance of his pinasters in particular show the propriety of planting them as a shelter to other trees. The following table shows the size attained by some of the pinasters and larches in twelve years after planting. The pinasters were seedlings of one year old, planted on very poor ground; the larches were three years old when planted, and the land of a better quality.

		CIRCUMFERENCE.						HEIGHT.	
		at the ground		3 ft. from grd.		6 ft. from grd.			
	No.	ft.	inch.	feet	inch.	feet	inch.	feet	inch.
Pinaster.	1	3	0	2	4	1	10	20	0
	2	2	8	2	0	1	4	17	0
	3	2	5	1	10	1	4	18	3
Larch.	1	3	0	2	0	1	7	24	6
	2	2	6	1	9	1	6	23	9
	3	2	5	1	8	1	5	23	11

For these plantations a second gold medal was adjudged to Dr. Bain this session.

Premiums for oaks.

The gold medal, being the premium offered in class 3 for raising oaks, was adjudged to Henry Andrews, Esq., of Wakefield; and, in consequence of the death of Mr. Andrews, the medal was presented to his two daughters. The oaks were planted with other trees, and the following is an account of the whole.

In

In February and March, 1809.		In Feb. and March, 1810.	Total.
Black Italian poplars	500	1000	1500
Huntingdon willows	1000	1000	2000
Ash	6000	5000	11000
Oaks	12000	10000	22000
Scotch firs	45000	48000	93000
Birch	8800	8000	16800
Larch	10000	20000	30000
Spruce	10000	20000	30000
Alders	1800	10000	11800
Sycamores	660		660
	95760	123000	218760

The first plantation was 36 acres, 3 rods, 10 perches; the second, 42 acres, 1 rod. The whole is well fenced with sod walls, five feet high, and three feet and half thick.

Premium for larches.

The gold medal was also adjudged to Wm. Congreve, Esq., of Aldermaston house, Berkshire, for planting 377520 larches, being the premium offered in class 10. He planted 108 acres in rows 3 feet asunder, and the plants at the same distance: 50 acres with the trees six feet asunder each way, except near the outsides, where they were only three feet; and 32 acres with the trees four feet distant each way, which distance he thinks preferable to any other. It is his intention to extend his plantations to 500 or 600 acres. Several of the last years shoots of a small plantation of larch, made in 1806, exceeded three feet in length, and one was three feet nine inches.

Second premium for larches.

The silver medal was voted to Mr. Henry Cowlishaw, of Mansfield, for planting 75000 larches, being the premium offered in class 11. The land is on Blidsworth forest, part of Sherwood. The following account is in his own words.

Management of the plantation.

The land being chiefly covered with heath from six to eighteen inches high, I caused a piece of the heath sod to be pared off with a paring-spade, of a sufficient space to plant the tree in; and the soil being very thin and near the gravel, I preferred planting the tree without turning over the soil.

The season being far advanced, and not having been sooner in possession of the land, I ordered that the roots of the trees should be made wet with water, and then rubbed over

 with

Plantation of larches.

with soil, which thus adhered to the roots; and in this state they were planted in the proportion of rather more than five thousand trees upon each acre, having planted seventy-five thousand trees upon the land, which is not more than fourteen acres, allowing for the fences.

The larch trees were two years transplanted, and from eight to fifteen inches high when planted out.

The season proved very favourable, few of the trees died, as one thousand filled up the deficiencies in the autumn of 1808, and the remainder grew well. In the autumn of 1809 they were again filled up with the same number; and I have this month supplied all the deficiencies with two thousand more, as some had been destroyed by rabbits.

The plantation is now in a healthy growing state; the last season it has much improved.

I think the above mode preferable either to destroying the heath, (as I presume it preserves the moisture in the soil during the summer, and affords warmth in the winter), or making holes by turning up the soil, and bringing what is bad upon the surface.

I am justified in these remarks from plantations adjoining mine, where both modes have been tried, and neither has answered so well as my method. My plantation is protected by a quick fence, which was planted in 1808, and secured by good posts and rails all round; the quicks have grown very well, considering the nature of the soil, which is but barren, and they are likely to become a good fence.

The following is an account of the expences that have attended this plantation.

	£	s.	d.
Purchase of the land and stamp	200	16	0
Seventy-nine thousand larches at £1. per thousand	79	0	0
Posts and rails............................	30	9	0
Paring, planting, and putting down the fences	38	0	0
Carriage of trees, &c......................	2	16	0
Cleaning the trees first and second year, where the heath in any measure incommoded them	2	11	0
Expenses of filling up the deficiencies	3	0	6
	356	12	6

Wernerian

Wernerian Natural History Society.

At the meeting on the 28th of March, professor Jameson read an account of a floetz gypsum formation, which occurs on the banks of the Whitadder, near Kelso. Likewise of a beautiful floetz quartz found in beds in the coal district of Fifeshire: and of the occurence of basalt, amygdaloid, and trap-tuff, in a coal-formation, newer than the old red sand-stone, and its accompanying porphyry, but probably older than the general mass of the rocks of the newest floetz-trap formation. At the same meeting, Mr. Leach read a description of the pig of Orkney and Shetland, which he is inclined to consider as a distinct species. And the Secretary laid before the meeting a very full and interesting thermometrical register and meterological journal, kept on a voyage to Davis Straits and back again, by Mr. John Aitkin, surgeon.

Mineralogy.

Species of pig.

Meteorological journal.

At the meeting on the 11th of April, Dr. Macknight read a mineralogical description of Tinto, a noted mountain in Lanarkshire. It appears to be of floetz formation; probably resting on the gray wacke, which pervades the whole mountainous districts in the south of Scotland. Around the base is found conglomerate, containing rounded masses of gray-wacke, iron clay, flinty slate, splintery horn-stone, quartz, felspar, mica, &c. Where the rock becomes finer grained, it approaches in some places to gray-wacke, and in others to those portions of the old red sand-stone formation, which are conjectured to alternate with the newer members of the transition series. Over the conglomerate, masses of clay-stone, greenstone, and green-stone passing into clinkstone, and porphyry-slate, successively appear, till we reach the summit, which, along with the whole of the upper part, is found to consist of compact felspar, and felspar porphyry. The disposition of the rocks in this mountain is conformable to the idea of secondary deposition, by assuming a finer and more crystalline texture as they ascend; and the occurrence of clay-stone and felspar in a position corresponding to what is observed on the Eildon Hills, the Pentlands, the Ochills, Papa Stour, Dundee, and in other places, seems to favour the hypothesis of a particular overlying formation, in which these

Mountain of Tinto.

these substances are prevailing ingredients, extending over a considerable portion of the lower country of Scotland.—In the bed of the Clyde, to the eastward of Tinto, amygdaloid appears, having nodules of calcedony coated with green earth; also calcspar, and portions of steatite.—Towards the north, the conglomerate forming the base of Tinto passes into the sandstone of which the whole inferior districts of Lanarkshire are composed. It is to the waste of this rock that we owe the splendid scenery of Cora Linn, and the other celebrated falls of the Clyde, a river which exhibits in its course many charms of nature, and may indeed be said to carry along with it beauty and fertility.

Meteorology of Hudson's Bay.

At the same meeting, the Secretary communicated a very curious meteorological journal, kept by Governor Graham, during his residence in Hudson's Bay.

Geological Society.

Substances distilled from wood analogous to bitumens.

May the 1st. A paper by Dr. Mac Culloch, M. G. S., on bistre and other substances produced in the distillation of wood; and on their analogy with the native bitumens, was read. When wood is submitted to destructive distillation, there is obtained, among other products, a black substance resembling common tar. This tar is very inflammable, and so liquid, that it may be burnt in a lamp. By washing it with water either hot or cold, or submitting it to the action of lime, or of the mild alkalis, a large portion of acetic acid is separated, and the residue becomes pitchy and tenacious. It is entirely soluble in caustic alkali, in alcohol, in ether, in acetic acid, and in the mineral acids. The fat oils and the recent essential oils dissolve but little of it, but if the former are made drying, and if the latter have become brown by keeping, they then act more readily and copiously. Coloured oil of turpentine takes up a considerable quantity, but naphtha only acquires a scarcely sensible brown colour, by digestion upon it. When carefully distilled at a gentle heat it is decomposed into an oily matter, at first limpid, and afterward brown, a quantity of acetic acid combined with a little ammonia, and a spungy coal remains in the retort. In this

process

process no inflammable gas is given out; but at a high temperature the oil is more or less decomposed, and inflammable gas is produced; which, however, does not burn with a flame by any means so bright as the gas from pit coal.

Residuum.

If this destructive distillation is not carried very far, the matter in the retort will be found, when cold, to be solid, brilliant, shining, and possessed of a conchoidal fracture: its taste is burning and pungent, and its odour is that of wood smoke. It is fusible and readily inflammable. When kept melted in an open vessel, till it ceases to be fusible, it becomes more and more brilliant, its fracture passes to splintery, and it assumes the perfect appearance of asphaltum. In proportion as it approaches this state it becomes less and less soluble in alcohol, and at length scarcely gives a stain to this mentruum. Naphtha has no action on it, and in this circumstance alone it differs from asphaltum.

Difference between bitumens and recent vegetable matter.

Dr. Mac Culloch then proceeds to an examination of the bitumens, and shows, that the difference between the products of recent vegetable matter and of the bitumens, when subjected to distillation, consists in the former yielding empyreumatic acetic acid, and a black pitchy matter insoluble in naphtha; while the latter afford ammonia and naphtha, but little or no acid.

Lignites examined.

He then enters into a detailed investigation of the properties of the very important class of lignites, or those substances such as peat, surturbrand, Bovey coal, &c. in which the traces of vegetable origin are not obliterated. Submerged wood from peat mosses gave a brown oil, smelling of wood tar, and refusing to dissolve in naphtha. A compact pitchy looking peat gave a fetid oil, resembling in odour neither wood tar nor bitumen, and very slightly soluble in naphtha. Bovey brown coal gave an oil resembling in odour that of wood tar, but much more soluble in naphtha. That portion of the oil which was insoluble in this menstruum had a strong odour of wood smoke. The oil of jet was almost perfectly soluble in napntha, and smelled strongly of pretroleum, but it afforded also empyreumatic acetic acid.

Thus

appears to be contained either in the unstratified trap, or in the slaty granwacke, nor did there occur in them, with the exception of one equivocal instance, the smallest trace of any organic remain.

Island of Teneriffe.

May the 15th.—An account of the Island of Teneriffe, by the Hon. Henry Grey Bennet, M. G. S. was read. The greatest length of this island from north to south is about 70 miles, its greatest breadth does not exceed 30 miles. In the S. W. part of the island is situate the mountain called by the Spaniards *el Pico di Tiede*, but better known by the name of the Peak of Teneriffe, the height of which, from the mean of several observations, appears to be about 12500 English feet. The rocks and strata of this island appear to be wholly volcanic. A long chain of mountains passes through the interior, sloping on the E. W. and N. sides to the sea, but on the S. and S. W. elevated into nearly perpendicular mountains, which are intersected by deep and narrow ravines. The lowest bed of the island is porphyritic lava, composed of hornblende and felspar, in its upper part porous, scoriform, and sometimes passing into the state of pumice. Upon this rests a bed of the same substance, as already mentioned, but in structure nearly approaching to greenstone. This is covered by a thick bed of pumice, which itself is overspread with basaltic lava, on which, in many places, rest beds of tufa and volcanic ashes. This basaltic lava decomposes sooner than any of the other rocks, and contains the greatest variety of imbedded substances: it is sometimes divided by a layer of olivine in crystals some inches long, and is often intersected by thick veins of porphyritic slate. Zeolite and chalcedony also occur in it. The number of small craters and extinct volcanoes is prodigious, They are to be found in all parts of the island; but none of them have been in activity of late years. The great streams of lava have flowed from the Peak: those of the years 1704 and 1797 (which was the last) are basaltic. This latter flowed so slowly, notwithstanding the steep descent of the mountain, that it was several days in advancing three miles. On the western side of the Peak is an ancient lava, not at all decomposed, several miles in length, and in a perfect state of vitrification resembling obsidian.

Mr. Vauquelin

Mineral water of Néris.

Mr. Vauquelin has analysed the thermal water of Néris, near Montluçon, in the department of the Allier. Two ounces of the solid matter left by evaporating the water on the spot had been sent to him: but he was not informed of how much water it was the produce. The results of his analysis were.

Carbonate of soda	33·34
Sulphate of soda	28·68
Muriate of soda	15·28
Carbonate of lime	2·80
Silex	8·34
Water	9·02
Animal matter, and loss	2·54
	100·

The silex he supposes to have been held in solution by the water; and he thinks it probable, that both this and the animal matter were indebted for their solubility to the presence of the carbonate of soda.

Mineral water of Argentières.

He likewise analysed the residuum of the water of Argentières, sent him in the same way by the same physician. The results were.

Carbonate of soda	32·08
Sulphate of soda	15·75
Muriate of soda	1·39
Siliceous sand	10·42
Carbonate of magnesia	34·37
———— lime	5·21
Animal matter	0·78
	100·

Mathematical Repository.

The Twelfth Number of Leybourn's Mathematical Repository contains—1. Solutions to the Mathematical Questions proposed in Number X. 2. On the irreducible Case of Cubic Equations. 3. New Properties of the Conic Sections. 4. Indeterminate Problems. 5. On the Ellipse and Hyperbola. 6. On the Roots of Equations of all Dimensions. 7. Properties of the Right-angled Triangle. 8. Continuation of Le Gendre's Memoir on Elliptic Transcendentals. 9. A series of new Questions to be answered in a subsequent number.

To Correspondents.

Dr. Henderson's paper is obliged to be postponed till next month.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1812.

ARTICLE I.

On a gaseous Compound of carbonic Oxide and Chlorine. By JOHN DAVY, *Esq. Communicated by Sir* HUMPHREY DAVY, *Knt. LL. D. Sec. R. S**.

SINCE the influence of electricity and solar light, as chemical agents, are analogous in many respects, and as the former produces no change in a mixture of carbonic oxide and chlorine, it was natural to infer the same respecting the latter. Messrs. Gay-Lussac and Thenard assert, that this is the case; they say, that they have exposed a mixture of carbonic oxide and chlorine, under all circumstances, to light, without observing any alteration to take place†; Mr. Murray has made a similar statement‡. Oximuriatic gas said not to act on carbonic oxide.

Having been led to repeat this experiment, from some objections made by the last mentioned gentleman to the theory of my brother, sir Humphry Davy, concerning chlorine, I was surprised at witnessing a different result. The contrary found by Mr. Davy.

The mixture exposed, consisted of about equal volumes of chlorine and carbonic oxide; the gasses had been previously Experiment.

* Philos. Trans for 1812, p. 144.

† Recherches Phisico-Chimiques, Tom. II, p. 150.

‡ Nicholson's Journal, vol. XXX, p. 227.

ously dried over mercury by the action of fused muriate of lime; and the exhausted glass globe, into which they were introduced from a receiver with suitable stopcocks, was carefully dried. After exposure for about a quarter of an hour to bright sunshine, the colour of the chlorine had entirely disappeared; the stopcock belonging to the globe being turned in mercury recently boiled, a considerable absorption took place, just equal to one half the volume of the mixture, and the residual gas possessed properties perfectly distinct from those belonging either to carbonic oxide or chlorine.

Properties of the resulting gas.

Thrown into the atmosphere, it did not fume. Its odour was different from that of chlorine, something like that which one might imagine would result from the smell of chlorine combined with that of ammonia, yet more intolerable and suffocating than chlorine itself, and affecting the eyes in a peculiar manner, producing a rapid flow of tears, and occasioning painful sensations.

Its chemical properties were not less decidedly marked, than its physical ones.

Thrown into a tube full of mercury containing a slip of dry litmus paper, it immediately rendered the paper red.

Mixed with ammoniacal gas, a rapid condensation took place, a white salt was formed, and much heat was produced.

The compound of this gas and ammonia was a perfect neutral salt, neither changing the colour of turmeric nor litmus; it had no perceptible odour, but a pungent saline taste; it was deliquescent, and of course very soluble in water; it was decomposed by the sulphuric, nitric, and phosphoric acids, and also by liquid muriatic acid; but it sublimed unaltered in the muriatic, carbonic, and sulphureous acid gasses, and dissolved without effervescing in acetic acid. The products of its decomposition collected over mercury were found to be the carbonic and muriatic acid gasses; and in the experiment with concentrated sulphuric acid, when accurate results could be obtained, these two gasses were in such proportions, that the volume of the latter was double that of the former.

Decomposd into carbonic and muriatic acid gasses.

Condenses 4 times its bulk

I have ascertained by repeated trials, both synthetical and analytical, that the gas condenses four times its volume of the

the volatile alkali, and I have not been able to combine it with a smaller proportion. of ammonia.

Tin fused in the gas in a bent glass tube over mercury, by means of a spirit lamp, rapidly decomposed it; the liquor of Libavius was formed; and when the vessel had cooled, there was not the least change of the volume of the gas perceptible; but the gas had entirely lost its offensive odour, and was merely carbonic oxide; for like carbonic oxide it burnt with a blue flame, afforded carbonic acid by its combustion, and was not absorbable by water. Decomposed by tin,

The effects of zinc, antimony, and arsenic heated in the gas, were similar to those of tin; compounds of these metals and chlorine were formed, and carbonic oxide in each experiment was liberated equal in volume to the gas decomposed. In each instance the action of the metal was quick; the decomposition being completed in less than ten minutes: but though the action was rapid, it was likewise tranquil, no explosion ever took place, and none of the metals became ignited or inflamed. zinc, antimony and arsenic,

The action even of potassium heated in the gas was not violent. But from the great absorption of gas, and from the precipitation of carbon indicated by the blackness produced, not only the new gas, but likewise the carbonic oxide, appeared to be decomposed. potassium,

The white oxide of zinc heated in the gas quickly decomposed it, just as readily indeed as the metal itself; there was the same formation of the butter of zinc; but instead of carbonic oxide being produced, carbonic acid was formed; and, as usual, there was no change of volume. white oxide of zinc,

The protoxide of antimony fused in the gas rapidly decomposed it; the butter of antimony and the infusible peroxide were formed; there was no change of the volume of the gas, and the residual gas was carbonic oxide. and protoxide of antimony.

Sulphur and phosphorus sublimed in the gas, produced no apparent change; the volume of the gas was unaltered, and its characteristic smell was undiminished. Not decomposed by sulphur, phosphorus,

Mixed with hidrogen or oxigen singly, the gas was not inflamed by the electric spark, but mixed with both, in proper proportions, viz. two parts in volume of the former and one of the latter to two parts of the gas, a violent explosion or hidrogen or oxigen singly:

was produced, and the muriatic and carbonic acid gasses were formed.

but quickly by water.

The gas transferred to water was quickly decomposed, the carbonic and muriatic acids were formed, as in the last experiment, and the effect was the same even when light was excluded.

Nature of the compound.

From the mode of the formation of the gas and the condensation that takes place at the time, from the results of the decomposition of its ammoniacal salt, and from the analysis of the gas by metals and metallic oxides, it appears to be a compound of carbonic oxide and chlorine condensed into half the space which they occupied separately.

Seemingly an acid.

And from its combining with ammonia, and forming with this alkali a neutral salt, and from its reddening litmus, it seems to be an acid. It is similar to acids in other respects; in decomposing the dry subcarbonate of ammonia, one part in volume of it expelling two parts of carbonic acid gas; and in being itself not expelled from ammonia by any of the acid gasses, or by acetic acid. Independant of these circumstances, were power of saturation to be taken as the measure of affinity, the attraction of this gas for ammonia must be allowed to be greater than that of any other substance, for its saturating power is greater; no acid condenses so large a proportion of ammonia; carbonic acid only condenses half as much, and yet does not form a neutral salt. The great saturating and neutralizing powers of this gas are singular circumstances, and particularly singular when compared with those of muriatic acid gas.

Its attraction for ammonia very great.

Its relation to the fixed alkalis not known.

In consequence of its being decomposed by water, I have not been able to ascertain whether it is capable of combining with the fixed alkalis. Added to solutions of these substances it was absorbed, and carbonic acid gas was disengaged by an acid.

It does not decompose carbonate of lime or barytes.

I have made the experiment on the native carbonates of lime and barytes, but the gas did not decompose these bodies. This indeed might be expected, since quick-lime, I find, does not absorb the gas: a cubic inch of it, exposed to the action of lime in a tube over mercury, was only diminished in two days to nine tenths of a cubic inch, and no further absorption was afterwards observed to take place.

But

But even this circumstance does not demonstrate, that the gas has no affinity for lime, and is not capable of combining with it; for on making a similar experiment with carbonic acid, substituting this gas for the new compound, the result was the same; in two days only about one tenth of a cubic inch was absorbed.

Not decomposed by spirit of wine.

Though the gas is decomposed by water, yet it appears to be absorbed unaltered by common spirit of wine, which contains so considerable a quantity of water; it imparted its peculiar odour to the spirit, and its property of affecting the eyes; five measures of the spirit condensed sixty measures of the gas.

Absorbed by the fuming liquor of arsenic andoximuriate of sulphur.

It is also absorbed by the fuming liquor of arsenic, and by the oximuriate of sulphur.

The former appeared to require for saturation ten times its own volume; six measures of the liquor condensed about sixty of the gas. The liquor thus impregnated was thrown into water, and a pretty appearance was produced by the sudden escape of bubbles of the gas; had not its intolerable smell convinced me that the gas was unaltered, I should not have conceived that it could pass through water undecomposed.

Passedthrough water undecomposed.

Difference of the author's results with others.

I cannot account for the assertion of Messrs. Gay-Lussac and Thenard and of Mr. Murray, that oximuriatic gas does not, when under the influence of light, exert any action on carbonic oxide: I was inclined at first to suppose, that the difference between their results and mine might be owing to their not having exposed the gasses together to bright sunshine; but I have been obliged to relinquish this idea, since I have found that bright sunshine is not essential, and that the combination is produced in less than twelve hours by the indirect solar rays, light alone being necessary.

The formation of the gas shown.

The formation of the new gas may be very readily witnessed, by making a mixture of dry carbonic oxide and chlorine in a glass tube over mercury: if light be excluded, the chlorine will be absorbed by the mercury, the carbonic oxide alone remaining; but if bright sunshine be immediately admitted when the mixture is first made, a rapid ascension of the mercury will take place, and in less than a minute the colour of the chlorine will be destroyed, and in about ten minutes

minutes the condensation will have ceased, and the combination of the two gasses will be complete.

Complete absence of water necessary.

It is requisite, that the gasses should be dried for forming this compound; if this precaution is neglected, the new gas will be far from pure: it will contain a considerable admixture of the carbonic and muriatic acid gasses, which are produced in consequence of the decomposition of hygrometrical water. Indeed there is considerable difficulty in procuring the new gas tolerably pure; a good air pump is required, and perfectly tight stopcocks, and dry gasses, and dry vessels.

Attempt to form it in a hot earthen tube.

I have endeavoured to procure the gas, by passing a mixture of carbonic oxide and chlorine through an earthen-ware tube heated to redness; but without success.

Its specific gravity.

The specific gravity of the gas may be inferred from the specific gravities of its constituent parts jointly with the condensation that takes place at their union. According to Cruickshank, 100 cubic inches of carbonic oxide weigh 29·6 grains; and according to Sir Humphry Davy, 100 of chlorine are equal to 76·37 grains: hence as equal volumes of these gasses combine, and become so condensed as to occupy only half the space they before filled, it follows that 100 cubic inches of the new compound gas are equal to 105·97 grains. Thus this gas exceeds most others as much in its density as it does in its saturating power.

Affinities of chlorine for hidrogen and carbonic oxide equal.

To ascertain whether chlorine has a stronger affinity for hidrogen than for carbonic oxide, I exposed a mixture of the three gasses in equal volumes to light. Both the new compound and muriatic acid gas were formed, and the affinities were so nicely balanced, that the chlorine was nearly equally divided between them. And that the attraction of chlorine for both these gasses is nearly the same, appears to be confirmed by muriatic acid not being decomposed by carbonic oxide, or the new gas by hidrogen.

Name for the new compound.

The chlorine and carbonic oxide are, it is evident from these last facts, united by strong attractions; and as the properties of the substance as a peculiar compound are well characterized, it will be necessary to designate it by some simple name. I venture to propose that of phosgene, or phosgene gas; from φως, light, and γινομαι, to produce, which signifies

nifies formed by light; and as yet no other mode of producing it has been discovered.

I have exposed mixtures consisting of different proportions of chlorine and carbonic acid to light, but have obtained no new compound. Oximuriatic and carbonic acid gasses will not combine.

The proportions in which bodies combine appear to be determined by fixed laws, which are exemplified in a variety of instances, and particularly in the present compound. Oxigen combines with twice its volume of hidrogen and twice its volume of carbonic oxide to form water and carbonic acid, and with half its volume of chlorine to form euchlorine; and chlorine reciprocally requires its own volume of hidrogen and its own volume of carbonic oxide to form muriatic acid and the new gas. Relative proportions of compounds.

This relation of proportions is one of the most beautiful parts of chemical philosophy, and that which promises fairest, when prosecuted, of raising chemistry to the state and certainty of a mathematical science.

II.

A Narrative of the Eruption of a Volcano in the Sea off the Island of St. Michael. By S. TILLARD, *Esq. Captain in the Royal Navy. Communicated by the Right Hon. Sir* JOSEPH BANKS, *Bart. K. B. P. R. S.**

APPROACHING the island of St. Michael's, on Sunday the 12th of June, 1811, in his majesty's sloop Sabrina, under my command, we occasionally observed, rising in the horizon, two or three columns of smoke, such as would have been occasioned by an action between two ships, to which cause we universally attributed its origin. This opinion was, however, in a very short time changed, from the smoke increasing and ascending in much larger bodies than could possibly have been produced by such an event; and having heard an account prior to our sailing from Lisbon, that in the preceding January or February a volcano had burst Smoke seen ascending from the sea, arising from a volcano.

* Philos. Trans. for 1812, p. 152.

burst out within the sea near St. Michael's, we immediately concluded, that the smoke we saw proceeded from this cause, and on our anchoring the next morning in the road of Ponta del Gada, we found this conjecture correct as to the cause, but not to the time; the eruption of January having totally subsided, and the present one having only burst forth two days prior to our approach, and about three miles distant from the one before alluded to.

Two at three miles distance.

Visit to the place.

Desirous of examining as minutely as possible a contention so extraordinary between two such powerful elements, I set off from the city of Ponta del Gada on the morning of the 14th, in company with Mr. Read, the consul general of the Azores, and two other gentlemen. After riding about twenty miles across the NW. end of the island of St. Michael's, we came to the edge of a cliff, whence the volcano burst suddenly upon our view in the most terrific and awful grandeur. It was only a short mile from the base of the cliff, which was nearly perpendicular, and formed the margin of the sea; this cliff being as nearly as I could judge from three to four hundred feet high. To give you an adequate idea of the scene by description is far beyond my powers; but for your satisfaction I shall attempt it.

The volcano described.

Imagine an immense body of smoke rising from the sea, the surface of which was marked by the silvery ripling of the waves, occasioned by the light and steady breezes incidental to those climates in summer. In a quiescent state, it had the appearance of a circular cloud revolving on the water like a horizontal wheel, in various and irregular involutions, expanding itself gradually on the lee side; when suddenly a column of the blackest cinders, ashes, and stones would shoot up in form of a spire at an angle of from ten to twenty degrees from a perpendicular line, the angle of inclination being universally to windward: this was rapidly succeeded by a second, third, and fourth, each acquiring greater velocity, and overtopping the other till they had attained an altitude as much above the level of our eye, as the sea was below it.

As the impetus with which the columns were severally propelled diminished, and their ascending motion had nearly ceased, they broke into various branches resembling a groupe of pines, these again forming themselves into festoons of white feathery

feathery smoke in the most fanciful manner imaginable, intermixed with the finest particles of falling ashes, which at one time assumed the appearance of innumerable plumes of black and white ostrich feathers surmounting each other; at another, that of the light wavy branches of a weeping willow.

During these bursts, the most vivid flashes of lightning continually issued from the densest part of the volcano; and the cloud of smoke now ascending to an altitude much above the highest point to which the ashes were projected, rolled off in large masses of fleecy clouds, gradually expanding themselves before the wind in a direction nearly horizontal, and drawing up to them a quantity of waterspouts, which formed a most beautiful and striking addition to the general appearance of the scene.

Water spouts drawn up by the clouds.

That part of the sea, where the volcano was situate, was upwards of thirty fathoms deep, and at the time of our viewing it the volcano was only four days old. Soon after our arrival on the cliff, a peasant observed he could discern a peak above the water: we looked, but could not see it; however, in less than half an hour it was plainly visible, and before we quitted the place, which was about three hours from the time of our arrival, a complete crater was formed above the water, not less than twenty feet high on the side where the greatest quantity of ashes fell; the diameter of the crater being apparently about four or five hundred feet.

Rising of the crater above the water.

The great eruptions were generally attended with a noise like the continued firing of cannon and musquetry intermixed, as also with slight shocks of earthquakes, several of which having been felt by my companions, but none by myself, I had become half sceptical, and thought their opinion arose merely from the force of imagination; but while we were sitting within five or six yards of the edge of the cliff, partaking of a slight repast which had been brought with us, and were all busily engaged, one of the most magnificent bursts took place which we had yet witnessed, accompanied by a very severe shock of an earthquake. The instantaneous and involuntary movement of each was to spring upon his feet, and I said "this admits of no doubt." The words had scarce passed my lips, before we observed a large portion of

Eruptions attended with earthquakes.

Fall of part of the cliff.

of the face of the cliff, about fifty yards on our left, falling, which it did with a violent crash. So soon as our first consternation had a little subsided, we removed about ten or a dozen yards farther from the edge of the cliff, and finished our dinner.

Farther account.

On the succeeding day, June 15th, having the consul and some other friends on board, I weighed, and proceeded with the ship towards the volcano, with the intention of witnessing a night view; but in this expectation we were greatly disappointed, from the wind freshening and the weather becoming thick and hazy, and also from the volcano itself being clearly more quiescent than it was the preceding day. It seldom emitted any lightning, but occasionally as much flame as may be seen to issue from the top of a glass-house, or foundery chimney.

On passing directly under the great cloud of smoke, about three or four miles distant from the volcano, the decks of the ship were covered with fine black ashes, which fell intermixed with small rain. We returned the next morning, and late on the evening of the same day, I took my leave of St. Michael's to complete my cruize.

On opening the volcano clear of the NW. part of the island, after dark on the 16th, we witnessed one or two eruptions that, had the ship been near enough, would have been awfully grand. It appeared one continued blaze of lightning; but the distance which it was at from the ship, upwards of twenty miles, prevented our seeing it with effect.

The volcano quiet, and 80 yards above water.

Returning again towards St. Michael's on the 4th of July, I was obliged, by the state of the wind, to pass with the ship very close to the island, which was now completely formed by the volcano, being nearly the height of Matlock High Tor, about eighty yards above the sea. At this time it was perfectly tranquil, which circumstance determined me to land, and explore it more narrowly.

Landing on the island.

I left the ship in one of the boats, accompanied by some of the officers. As we approached, we perceived it was still smoking in many parts, and upon our reaching the island found the surf on the beach very high. Rowing round to th[illegible]ide, with some little difficulty, by the aid of an oar, I jumped on shore, and was followed by the other officers.

officers. We found a narrow beach of black ashes, from which the side of the island rose in general too steep to admit of our ascending; and where we could have clambered up, the mass of matter was much too hot to allow our proceeding more than a few yards in the ascent.

The declivity below the surface of the sea was equally steep, having seven fathoms water, scarce the boat's length from the shore, and at the distance of twenty or thirty yards we sounded twenty-five fathoms.

From walking round it, in about twelve minutes, I should judge that it was something less than a mile in circumference; but the most extraordinary part was the crater, the mouth of which, on the side facing St. Michael's, was nearly level with the sea. It was filled with water, at that time boiling, and was emptying itself into the sea, by a small stream about six yards over, and by which I should suppose it was continually filled again at high water. This stream, close to the edge of the sea, was so hot, as only to admit the finger to be dipped suddenly in, and taken out again immediately.

Less than a mile round.

The crater full of boiling water.

It appeared evident, by the formation of this part of the island, that the sea had, during the eruptions, broke into the crater in two places, as the east side of the small stream was bounded by a precipice, a cliff between twenty and thirty feet high forming a peninsula of about the same dimensions in width, and from fifty to sixty feet long, connected with the other part of the island by a narrow ridge of cinders and lava, as an isthmus of from forty to fifty feet in length, from which the crater rose in the form of an amphitheatre.

A peninsula joining the main island.

This cliff, at two or three miles distance from the island, had the appearance of a work of art resembling a small fort or block house. The top of this we were determined, if possible, to attain; but the difficulty we had to encounter in doing so was considerable; the only way to attempt it was up the side of the isthmus, which was so steep, that the only mode by which we could effect it, was by fixing the end of an oar at the base, with the assistance of which we forced ourselves up in nearly a backward direction.

Ascent of the isthmus.

Having reached the summit of the isthmus, we found another difficulty, for it was impossible to walk upon it, as the descent on the other side was immediate, and as steep as the one

one we had ascended; but by throwing our legs across it, as would be done on the ridge of a house, and moving ourselves forward by our hands, we at length reached that part of it where it gradually widened itself and formed the summit of the cliff, which we found to have a perfectly flat surface, of the dimensions before stated.

Flag planted, and the island named Sabrina.

Judging this to be the most conspicuous situation, we here planted the Union, and left a bottle sealed up containing a small account of the origin of the island, and of our having landed upon it, and naming it Sabrina Island.

Fishes destroyed by the eruption.

Within the crater I found the complete skeleton of a guard-fish, the bones of which, being perfectly burnt, fell to pieces upon attempting to take them up; and by the account of the inhabitants on the coast of St. Michael's, great numbers of fish had been destroyed during the early part of the eruption, as large quantities, probably suffocated or poisoned, were occasionally found drifted into the small inlets or bays.

Nature of the island.

The island, like other volcanic productions, is composed principally of porous substances, and generally burnt to complete cinders, with occasional masses of a stone, which I should suppose to be a mixture of iron and lime-stone; but have sent you specimens to enable you to form a better judgment than you possibly can by any description of mine.

III.

New Method of making Bricks, so as to form cheaper and firmer Buildings, and useful underground Drains: by JOHN STEPHENS, *Esq. of Reading, in Berkshire**.

SIR,

Bricks divided nearly through.

I HAVE sent, for the inspection of the Society of Arts, &c. three closure bricks, which on examination you will find to have been cut three fourths of the way through in the middle

* Trans. of the Soc. of Arts, vol. XXIX, p. 39. The silver medal was voted to Mr. Stephens.

by

by a wire, and the whole of the way through at each end, which leaves the ends square and handsome for work.

The bricklayer, to divide each brick in length, has only to take the brick in his left hand, with the mark, or cut, downwards longitudinally; and by one smart blow with the trowel he will have two complete king-closures, with which he can easily make four common closures. easily cut for closures.

I have shown them to many workmen, who all approve of them. I had two hundred and fifty of them made by a brick-maker for an experiment, and I have ordered two thousand more. The builders who do the principal part of my work have had some on their own account, and have since increased their orders. I have no doubt when they are better known they will come into general use.

A considerable saving in labour and waste of bricks may be effected by their use, particularly in walls where piers are built, and where there are many openings; the work will also be rendered more substantial. There will be a saving in room and materials where the back of a chimney is built against a straight wall, particularly in flues for low buildings. They will be found useful in cities or large towns by being placed in partition walls instead of lath and plaster, and be a check to the ravages of fire. They will be useful in preventing the passage of rats and mice, and the disagreeable smell occasioned when they die betwixt lath and plaster or wainscot. They will also answer for draining of land, and will form cheaper small drains from houses than any other method. They may be cut in other forms or directions for particular purposes according to the uses for which they are intended. Their advantages.

The additional expense of dividing them by the wire is about two shillings per thousand, it is generally done after they have been moulded one or two days according to the dryness of the season. Additional expense of making.

I flatter myself, that, if this communication meets with the approbation of the Society, it will render a benefit to the public. I am, Sir, with much esteem,

Your most obedient servant,

JOHN STEPHENS.

Reading, October 31, 1810.

DEAR

DEAR SIR,

Saving in their use.

On inquiry from builders, I am informed, that the saving by the use of the bricks I have invented will be from two and a half to nearly five per cent, in a five-window house in brick work and labour, in a front of forty feet with or without piers. In ornamental brick piers for gateways, I think the saving of bricks by means of cutting may be very considerable, and in the labour still more, beside the work being done more sound and substantial.

11 inch wall.

I am using a few of them in an eleven inch brick wall, (a system hitherto entirely new), in a westernly aspect, as a preventive or guard against the effects of weather, and it will, in point of dryness, be equal to a fourteen inch wall. I have enclosed a letter from Benjamin Garroway, a bricklayer, who has requested me to let him have all the bricks I have of this kind, and to bespeak more for him. I have also sent a certificate from Mr. Robert Wright, who is extensively engaged in buildings.

Drains.

The drains for agricultural purposes might be done by women or children, except the digging of the drains, especially two inch drains. With respect to longer drains, if they are required of four inches, and to be covered with brick, I would recommend the bricks to be laid anglewise, in order to promote strength in covering.

Drawback of duty.

It would be of great importance if parliament would allow a drawback of the duty on all bricks employed in draining.

Mode of making the brick.

Every brick, intended for the operation I recommend, is taken off the stack two or three days after it is moulded. It is then put on a stool or board, and a wire, about the size of No. 23, is pressed on the upper side of the brick, so as to pass through each end of it; it is then immediately placed on the stack again, and afterward burned.

I am, dear sir,

Your most obedient humble servant,

Reading, December 8, 1810. JOHN STEPHENS.

Letter from Mr. RICHARD BILLING, *to Mr.* STEPHENS.

SIR,

Remarks on the utility of these bricks.

Agreeably to your request, I have taken into consideration the utility of your closure bricks, and beg leave to say, that

that my opinion coincides with yours, as to their advantage, in new chimnies, which are intended to be built against old walls. In constructing a new chimney it is generally considered absolutely necessary, that the same should be worked up close to the old wall, but completely unconnected, in order that it might settle from the old; in this case, it is very desirable to make the back of the chimney as thin as possible, that it may project as little as convenient; and in building piers, particularly small ones, either for gate-ways or fronts of houses, where there are many bricks, and in the present mode, which is so frequently adopted, of two inch recesses at the exterior of the windows, your closures would be much preferable even in appearance to a brick which has been cut with a trowel, with the surface of course defaced.

Drains.

Closure bricks might be adopted as a cheap and useful drain by a common brick flat, with two closures laid on the same two inches asunder, or four inches, and reversed.

Ornamental work.

Your closures would be useful in all kinds of ornamental brick work.

Two inch bricks desirable.

Two inches is a very desirable brick, but most times avoided in consequence of the waste in cutting common bricks, and difficulty in producing a smooth face, which would be completely obviated by the introduction of closure bricks.

I remain, sir,
Your obedient humble Servant.
RICHARD BILLING.

Reading,
December 3, 1810.

Letter from Mr. BENJAMIN GARROWAY, *to Mr.* STEPHENS.

SIR,

Farther remarks.

I am of opinion, that, if closures were made for general use, two and a half per cent would be saved in brickwork of small piers, flues of chimneys, or where there are any bricks in ornamental works. Common bricks frequently will not cut more than one closure; and if your bricks were to be always had, they would be much more useful.

I remain, sir,
Your humble servant,
BENJAMIN GARROWAY.

Ruscombe,
December 5, 1818.

Letter

Letter from Mr. ROBERT WRIGHT *to Mr.* STEPHENS.

SIR,

Having examined your method of cutting bricks, I am of opinion, that they would be particularly useful in all kinds of brick work, make a considerable saving in labour and materials, and that a much superior bond would be obtained by your improvement.

You are perfectly at liberty to make any use you please of my opinion.

I am, sir,

Your very obedient servant,

ROBERT WRIGHT.

No. 5, New North Street, Red Lion Square, London, December 6, 1810.

Description of the drawings of Mr. Stephens's method of cutting bricks for various purposes. See Plate VI, fig. 2—7.

Explanation of the plate.

Fig. 2, of plate VI, is a plan of the upper surface of a common brick: the line *a a* is a cleft cut nearly through the brick while it is soft by means of a piece of wire, as is shown in the section, fig. 3; where the section of brick is shown at B B, placed on the wooden block A, a piece of wire *b b* with a loop at each end is pressed down into it, so as to divide it into two parts, except the part C, which the wire will not cut through because of the curvature it acquires in being pressed into the brick. A brick of this kind, being burnt, may be broken in two halves by one cleft with the trowel, which will be found very useful in many cases which constantly occur in brickwork, and will be far superior to the present mode of hacking the bricks, both for the soundness and appearance of the work, and will be done in less time.

Figs. 4 and 5 show the application of these divided bricks to draining, where A B are the ends of the two halves of a brick, and C D tiles, forming the top and bottom of the drain, this method forms a square drain.

Fig. 5 shows how a triangular drain may be made with half the number of bricks of the foregoing, that is one half brick A, and two tiles C D.

Fig.

Fig. 6, is a plan of a brick divided diagonally, and fig. 7 shows how these halves may be disposed to form a triangular drain: the letters show the same parts in each of these two figures: the bottom, D, may be made of tile, or of a brick cut in half in its thickness: the scale annexed to the figures will show the dimensions of the different drains.

IV.

A temporary Rick, to secure Corn in Sheaves in the Fields till quite dry; also Clover, Pease, and Beans: by WILLIAM JONES, *Esq. of Foxdown Hill, near Wellington, Somersetshire**.

SIR,

THE very unusual quantity of rain, that fell during the months of August and September last, with scarcely two days of dry weather following, in this neighbourhood, put farmers to the necessity of having recourse to various modes of preserving their corn; and, as I understand the Society of Arts has offered a gold medal for the cheapest and best mode of harvesting corn, and also for making hay in wet weather, superior to any hitherto practised, I beg leave to communicate some experiments I made last summer, and the result of them. In the first place, I put some wheat in small round ricks, or wind-rows, made in the common way of this county; but afterward recollected, that the uncommon wetness of the ground might render the under part damp. I thought it prudent to examine them, (about ten days after they were set up), and found my apprehensions so well founded, that I had the whole spread abroad; and have no doubt, that, if they had remained a little longer, the corn would have been materially injured; not the bottom only, for it had contracted dampness a great way up the ricks, insomuch that I turned my attention to devise some better mode of preserving my barley in case the weather continued so rainy, as it afterward proved. I had observed in some wet

Harvesting in wet weather.

Small ricks of wheat

injured by the dampness of the ground.

* Trans. of the Soc. of Arts. vol. XXIX, p. 46. The silver medal was voted to Mr. Jones for his invention.

Barley the same, seasons before this, that many of our farmers, not being able to get their barley dry enough to put into a large rick, had set up narrow ricks, containing the produce of an acre or two, each in different parts of the field where it was grown, for the sake of expedition; and though some straw was put under them, yet the bottom contracted a great degree of dampness, so as to occasion it to smell old, and the clover was killed where these ricks had stood. and the clover killed underneath.

Method of obviating these inconveniences. My object was to prevent both these injuries; and it occurred to me, that four gate hurdles would answer both purposes, by setting the two outside ones perpendicular, and two middle ones inclining against and supporting each other.

Stand for the rick. These hurdles are usually eight feet long; the two heads, in which the four bars are mortised, have pointed heads of about a foot and a half long; the two outside ones are to be forced into the ground nearly their full length, so that the middle brace may rest on the ground to afford some support; and the two middle ones about six inches, to keep them steady. The foot of the second hurdle should be set two feet from the foot of the first, the third three feet from the second, and the fourth two feet from the third, making seven feet, and occupying a space of seven feet by eight, for barley or oats; but wheat, being longer in the straw, requires the distance to be wider, viz. three feet from the first to the second, three feet from the second to the third, and three feet from the third to the fourth, which will be nine feet by eight.

It will be proper to put seven or eight small stakes, (a little bigger than a man's thumb), from the second bar of the first hurdle to the second bar of the second hurdle, and from the second bar of the third to the second bar of the fourth, to support the sheaves from the ground, to admit air under and prevent injury to the growing clover; or small poles may be used extending from one outside hurdle to the other. The appearance of the ends of the hurdles will be as in the engraved plan, Plate VI, fig. 1, and section Plate VII, fig. 1, which show where the small stakes are to be placed to prevent the sheaves touching the ground, for there will be but a slight pressure on them, since the ground ends of the sheaves are to be put against the hurdles A B, and the

Structure of the rick.

ears

ears of the corn a little elevated to rest against the hurdles C D: so that the ears of the corn will be all within side, and have the benefit of the air between C and D. It is to be observed, that the hurdles C D, being but six inches in the ground, and the hurdles A B nearly eighteen inches, the two former will be a little higher than the two latter; which is necessary for two reasons, one is, that the higher these are, the higher the air is admitted to the middle of the rick, and the more they elevate the tops of the sheaves in the middle, for the ground ends should be lowest to shoot off the rain. But as it will be found, that, after two or three rows are placed around the tops of the hurdles, (for the ricks should be circular), the ground ends of the sheaves being largest, the tops will become nearly level; when it will be necessary to put four sheaves as at G G in the middle horizontally, forming a square, open in the centre, which will admit air from the top of the middle hurdles C D, through this space, to the middle of the rick, as the ears of each sheaf are just to meet only in the middle resting on these four sheaves*; which will give such an elevation to the tops of them, that the ground ends will be sufficiently inclining downwards to shoot off any rain that may fall. In forming the roof, the sheaves are of course to be put farther in every time they are put around, till the roof terminates in a point, when two sheaves, with the tops downward spread abroad and bound with a straw band, will secure it from a great deal of rain; but if the corn is to remain out long, a little reed or thatch may soon be put on each rick.

Model.

Fearing I might not have been sufficiently explicit in describing this plan, it has occurred to me, that it would be better to send a model, containing 100 sheaves, made to a scale of an inch to a foot, as to the length of the hurdles, the distance from each other, and the size of the sheaves, also to exemplify every particular of it.

Barley saved in this manner,

The weather being so rainy for some days after my barley was cut, with every appearance of more rain, I determined,

* If the corn should be very damp, and the rick made high, four other sheaves may be put higher up to convey a greater circulation of air, and operate as a bond to connect the sheaves in the middle, so that they cannot possibly slide outwards.

on

on having a few hours intermission of rain, to get the middle of the field, which was a little more dry than the rest, and to put it in small ricks, containing more than the produce of an acre, on these hurdles in the same field; it was in such a damp state as to be totally spoiled in the common rick, but was taken from these ricks into a barn in the month of January last, perfectly dry, the straw much better than could have been expected, the grain good, having been proved to grow well; for having some doubt on account of being put together so damp, I had it first tried by putting a few grains in a cloth into the earth, and have since sown it, and no other this spring, and I never had a better prospect of a good crop. The remaining part of the barley, that was left on the ground, was not taken in till ten days afterwards, the grain much grown, a great deal wasted by frequently turning, and the straw spoiled.

while some other was spoiled.

I flatter myself it will be admitted, that in wet seasons, or when harvest is so late, that, as the days decrease, the dews increase, and of course remain so long that there are but few hours in a day for drying, even if there should be no rain; this method will afford perfect security to corn that is cut dry, and put up in this manner immediately from the sithe or sickle: because, if there should be grass in it, the ground end of every sheaf will be withoutside, exposed to the sun and air to dry; and as for the grain, no part of it can get damp, because the ears but just meet in the middle, through which the air passes from the bottom to the top sufficiently to dry it. I have mentioned sheaves, because in this county barley and oats are generally bound as well as wheat; but both the former may be placed in these ricks without binding, as I had some barley put in one of them (by way of experiment), and think it to be the better mode when there is much grass in it, by carefully keeping the ears together when carried to the hurdles, where a man is ready to put it up to another on the top, and to place the ears inwards; and it is done in as short a time as the like quantity is put on a waggon, with this advantage, that, whereas a waggon with three or four horses goes over the clover to the great injury of it in wet weather, by this method the corn is carried by women or children in their arms to the hurdles, without the

Barley and oats made into sheaves, but may be stacked without it.

No waggon to injure the clover.

the least injury to the clover, a consideration fully adequate to a little extra expense, if any, beside that of being more expeditiously secured; for every practical farmer will be sensible in how short a time an acre of corn may be carried from the circumference of an acre to its centre. As to the time of fixing these hurdles, I have ascertained, that two people can fix them in five minutes, and one rick would contain the produce of two acres of barley or oats. The other advantages, beside the corn being thus sooner secured, are, that no more attendance on it is required, so that a farmer's attention may be better directed to his other harvest concerns, and, that one or two of these ricks at a time, (as may be convenient), may be taken into a barn to thrash, whereas a part of a large rick cannot be taken in without the trouble and expense of thatching the remainder, and being subject to the risk of rain before it may be covered again.

Time of fixing the stand.

Other advantages.

I trust it will be seen, that by this plan there must be a great saving of the quantity as well as the preservation of the quality of the grain, which is known often times to shed a great deal by being frequently turned to get dry. Before I thought of this expedient (last barley harvest), I am clear, that a field of pease of mine required to be turned so often, that more shed out than were sown; and a farmer in this neighbourhood had a good crop of eight acres of vetches reduced to sixty bushels, by so frequently turning them for three weeks, without getting them dry at last; whereas an acre or two might have been taken up in this way a few days after they were cut, and the seed would have got sufficiently hard, but the greater part of these were so soft as to be much bruised in thrashing, and it was to be feared a great part of them would not vegetate. I had an opportunity of knowing the quantity, having the tithe of them; and proving the injury by the loss of my crop in sowing them, insomuch that the land has been since ploughed.

Saving of grain, as well as preserving from injury.

Although I have not tried it, yet I think it is not to be doubted, but that this mode may be applied with equal advantage to clover hay, and clover seed, before it may be dry enough to put into a large rick, by being placed in this situation to dry without being so frequently turned as to deprive the hay of its finest parts, and subject the seed to great waste.

Application to clover for hay or seed

and to grass cut for hay.

waste. In cases also when meadow hay may be dry enough to put in large cocks on the appearance of rain, how much injury do they receive by the bottom being rendered so wet as to occasion a dampness some way up, and require much time to throw abroad to dry? Whereas, in the same state of dryness, how many of such cocks may be put on four hurdles; and the bottom instead of being wet and injured will be perfectly dry, having air circulating under it, and from the two middle hurdles quite to the top; if a sheaf of reed was to be drawn up through it, as the hay got higher: a bundle of straw on the top would secure it from rain. Or, instead of a reedsheaf drawn up, a couple of small faggots of wood, or three or four poles bound together, and placed horizontally about the middle of the rick, to admit air at each end, and render it dry enough to be carried on to a rick without farther trouble or risk.

Hay injured by exposure to too much sun.

Hay is known to receive injury, not only from rain, but even from fervent sunshine, when nearly dry, if not frequently turned: as may be observed by the change of colour and loss of smell, which many farmers in this neighbourhood experienced in the summer of 1809, for want of hands to turn it sufficiently. I have seen an infusion of such hay made in a tea-pot, and compared with an infusion of the like quantity of good hay in another: the former was very deficient both in colour and taste to the latter, and the quality of it, of course, much deteriorated.

The straw improved.

We know that straw, particularly of barley or oats, will be much injured by being long on the ground exposed to soaking dews, and perhaps to alternate rain and sunshine; and may it not, when protected from them by this mode, be far superior for cattle to what we are at present aware of? Beside the advantages of grain, hay, and straw, being thus better preserved, and less expense of labour than by repeatedly turning in rainy seasons, there is another advantage of no small consequence, that the crops may be removed, and put on hurdles in another field, (without any hindrance to sheep feeding therein) when the land whence they were taken may be immediately ploughed; for instance, after pease, to facilitate a better fallow, (than if delayed), to be succeeded by wheat, and ploughing clover lays for wheat,

Farther advantage.

and

and also preparing land for turnips after vetches, to accelerate the sowing; in which case, the delay of a few days has frequently occasioned a total loss of the crop. Cheapness and

It is an essential consideration, that the expense attending improvements should not counterbalance their utility: and I flatter myself, there can be no objection to this mode on that score, because gate hurdles are useful appendages to a farm, in any county, for other purposes, when not used on this occasion; and in this and other counties they are requisite for dividing turnips for sheep; and, as to expedition, which is of great importance in harvest concerns, four of these hurdles (as I have already observed) may be fixed in five minutes. expedition.

If, therefore, the Society for the Encouragement of Arts, Manufactures, and Commerce, instituted for the laudable purposes which it professes, should think my plan combines utility with cheapness and expedition, I should consider myself flattered by their approbation; and feel a degree of satisfaction in the reflection, that I have not turned my thoughts in vain to a subject, which must be allowed to be of great importance. I am, sir,

Your most obedient servant,

Foxdown Hill, June 7th, 1810. W. JONES.

SIR,

I have been favoured with your letter, acknowledging your having received my model of a temporary rick, and recommending me to send certificates of its use.

I have to add, that the barley I had put on these hurdles last year was done in my presence, by the same man who removed it afterward to the barn, thrashed, and sowed it; he is ready to attest my former statement of the hurdles requiring only five minutes time to fix in the ground; of the barley preserved by them growing perfectly well, with a prospect, from its present appearance, of yielding a good crop; and with this farther remark, that it was so damp when put upon the hurdles, that he was apprehensive it would be spoiled, and was much surprised when he took it into the barn, to find it so perfectly dry. State of the barley saved.

I notice your query, whether these hurdles could not be applied to the purpose of temporary hovels for sheep, in Temporary hovel for sheep.

wet

wet weather? I think, that if two of them were fixed eight feet apart, and two others placed on the top of them, covered with straw, reed, rushes, heath, or furze, they would form a covered hovel of eight feet square, and afford great protection to sheep in wet weather, (particularly just after being shorn;) and to ewes in the lambing season also, if some, that were the most forward with lamb, were selected and put into enclosures, where one end of each hurdle might be put against a hedge, or against a wall, or end of a hovel. These hurdles, covered in like manner also, would be useful, if a number of them, proportioned to the quantity of sheep, were put in the form of a square, in any part of a field, in hot weather, to afford shade. They would induce the sheep to lie there, and answer the purpose of folding, as they could easily be moved to such part of the field as wanted improvement; and the sheep would be more at ease than when creeping under hedges, to the no small detriment of their wool.

Farther use of the temporary stand.

I have to report to the Society, that I have this harvest made use of the hurdles on a larger scale, viz. to keep raking wheat separate from the sheaf, and which was too damp to put in sheaf; and also in small ricks of wheat for seed, to save the trouble of taking it from a larger rick, before the whole was wanted to be thrashed; and for my tithe wheat, that was not sufficiently dry to put into a barn.

Late crop of pease.

I had also five acres of white pease, which were drilled where a crop of vetches had failed, so late as the 12th of May; they proved to be a very great crop, but they ripened so late, and the tops of the haulm were so green, from having shot out to an extraordinary length, that they were not all carried till the 27th of last month. At one time I almost despaired of ever getting them dry, owing to the heavy dews which fell during the night, and continued during most of the day, so as to afford but a few hours to dry my crop. I therefore took up six waggon loads from the middle of the field, on the 25th of last month, and put them on twelve gate hurdles adjoining each other, for the purpose of making one roof, and set the hurdles in the manner of my ricks. The first two loads were put on four of these hurdles at one end, which would contain four loads if

if necessary; the next two on the adjoining four hurdles; and the other two loads on the four remaining hurdles; so that though these three ricks were close to each other, yet being set up separately, they admitted air between each, from the bottom to the top, and yet adjoined sufficiently to make one continued roof to be thatched together.

When these six loads were removed from the field, I had room to turn the remaining parcels towards each other, and more towards the middle of the field, so as to have more air to dry. But they were not sufficiently dry till the 27th, when they were carried to another set of sixteen hurdles ready to take them, and each waggon load laid over the whole length of 16 hurdles, not being so damp as to require being carried up in separate ricks, as the former six loads. Some of these pease have been already thrashed, and prove to be in very good condition, as also the haulm, which is perfectly dry and sweet for cattle.

One of these ricks of pease, and probably some of the ricks of wheat, will not be taken in till the month of February next: they may therefore be inspected by any member of the Society, who may visit this neighbourhood.

I have enclosed a certificate from Mr. Waldron, a gentleman of this parish, who farms his own estate; and another certificate from Mr. Hewitt, also of this parish, who is esteemed a respectable and intelligent farmer; he rents a farm from Mr. Ware, brother to Mr. Ware of the house of Ware, Bruce, and Co. London.

I am, sir,
Your most obedient servant,
W. JONES.

Foxdown Hill, Oct. 30*th*, 1810.

SIR,

Agreeably to your request, I lose no time to give you the information you desire, respecting the temporary corn ricks, and the size they may be made. The space between the two outside hurdles contains about sixty sheaves on each side, or one hundred and twenty in the whole, to reach the top of the hurdles. Every round of sheaves afterward takes forty sheaves or upwards, say fifteen rounds high, which makes six hundred sheaves, and which will raise the rick

Size of temporary corn rick.

rick about eight feet from the tops of the hurdles. It will require about seventy sheaves from the top of the above fifteen rounds, to the top of the conic roof. Four sheaves crossing each other, five times in the centre of the rick, will form in the whole twenty, making as follows:

120	Sheaves to the top of the hurdles.
600	Sheaves from the tops of the hurdles, to the commencement of the roof.
72	Sheaves in the conical roofs.
20	Sheaves in the cross or bonds of the rick.
812	Sheaves, or upwards of 81 shocks in each rick, which is more than the average produce of an acre.

Length of sheaves.

The wheat in this part of the country is reaped near to the ground, and my sheaves, this year, are about $4\frac{1}{2}$ feet long, for which the distance of 3 feet $2\frac{1}{2}$ inches between the outer hurdles, and 3 feet between the inner hurdles, is calculated. The distance should be regulated by the length of the sheaves of barley and oats. When shorter than four feet, the rick should be oblong instead of round.

Faggots useful at intervals.

Faggots of wood, placed at intervals within the rick, will be found particularly useful, where pease, vetches, clover, hay seeds, and meadow hay, are put into these ricks, as the faggots will promote a greater circulation of air.

The number of the cross sheaves should be according to the dampness or dryness of the corn, either in every row, or every second or third row.

Reference to Plate VII, *fig.* 1, *the Section of Mr. Jones's Temporary Corn Rick.*

The letters, describing the same parts of the construction of the rick, agree with those in Plate VI.

A B, the two upright outside hurdles.

C D, the two inclined hurdles.

E E, the poles or sticks on which the sheaves are to be first placed on commencing the rick, and which cross the hurdles.

H H H H, the sheaves composing the body of the stack.

I I,

11, the conical roof, the lower part of which projects sufficiently over the body of the rick, to cover it from wet, and in this roof, each round of sheaves is to be placed so as to cover the ears of the sheaves below, and gradually rise to nearly a point, over which a bundle, containing two or three sheaves, with the butt ends upwards, and tied together, cover the centre or uppermost point of the rick.

V.

Improvement in the Acorn Dibble; by Mr. CHARLES WAISTELL, *of High Holborn.**

SIR,

IN consequence of information, that Government wanted intelligence respecting the best mode of dibbling acorns, I have made an improvement on the acorn dibble in the Society's repository, which I presume will answer well the desired purpose. I therefore send herewith a drawing of it, requesting you will have the goodness to lay it before the Society of Arts &c.

Dibble for acorns.

Thorn bushes and thickets are the natural guardians of young oaks from the depredations of cattle of all kinds, on forests and other grounds on which they pasture. By means of this implement, acorns may be deposited in the interior of bushes, as well as in open grounds, with rapidity and accuracy. And presuming that such an implement would be of great utility to many individuals, and also to Government, I wish much to have it made known as generally as possible among those who are most likely to profit by it; and which I think may be best effected by the Society of Arts, &c. giving an engraving of it in their next volume; provided they concur with me in thinking it may be the means of rearing an increased number of oaks, to promote which every possible facility should be given.

Bushes and thickets the natural guardians of young oaks.

Permit me on this occasion to observe, that many proprietors of landed property are not sufficiently aware, that a greater or less proportion of almost every estate would, if judiciously

Planting on estates not sufficiently attended to.

* Trans. of the Soc. of Arts, vol. XXIX, p. 60.

diciously planted, pay the proprietor much more than the rent it could be let for to a farmer. It would, therefore, give me great pleasure to see in the Society's volumes more communications from successful planters. I trust there are numerous persons of this description, who want only to be reminded, how greatly they might benefit individuals, as well as their country, by publishing or communicating to you such well ascertained facts of their success as planters, as they may be in possession of; and in order to direct their attention to the nature of the information that is chiefly wanted, I beg leave to refer them to pages 80 and 81 of the Society's 27th volume*, wherein numerous particulars respecting the planting, management, and produce of woods, are enumerated.

I am, Sir,

Your obedient servant,

CHARLES WAISTELL.

No. 99, High Holborn,
June 12, 1811.

Reference to the Engravings and Section of Mr. Waistell's Improvement of the Dibble for Planting Acorns. Plate VII, *figs.* 2, 3 *and* 4.

The dibble described.

a represents the handle of the dibble, which dibble is a rod ¾ of an inch in diameter, movable in the tube of a stave, which stave is externally about two inches diameter; *b* a tin or metal tube fixed on the exterior part of the stave, and of the same bore or aperture as the tube of the stave. When a hole is made in the earth by the point of the dibble *d*, the acorn is dropped down the metal tube, and on drawing up the dibble by its handle to the height of the letter *e*, the acorn *c* passes through a large opening into the dibble tube, and thence falls into the hole made by the point of the dibble in the earth; when by moving backwards and forwards the cross handles *gg*, fixed on the top of the hollow stave, the soil surrounding the hole in the earth is loosened by the iron wings *ff*, and deposited on the acorn. Fig. 4, *h*, shows a section of the iron wings *ff* belonging to the bottom of the hollow stave.

Method of using it.

Supposing that you wish to plant an acorn in the middle of any bush, you are to press the instrument through it into the

* See Journal, vol. XXVII, p. 307.

the ground, make a hole in the earth by the point of the dibble rod, then raise the rod above the hole where the two tubes communicate, drop the acorn down the tube *b*, which falls immediately through it and the lower part of the stave tube into the hole previously made by the rod, which hole is instantly covered by the soil raised by the wings. The dibble rod may be occasionally passed down the metal tube, to be certain of its being perfectly clear.

VI.

On the apparent Streaks of Light, left sometimes by falling or shooting Stars; and on their apparent rectilinear Courses in the Atmosphere. In a Letter from JOHN FAREY, *Sen. Esq.*

To W. NICHOLSON, Esq.

SIR,

SOME months ago I was induced, by the frequent references to shooting or falling stars, as being a phenomenon in and connected with particular states of our atmosphere, that I had noticed in the improved Meteorological Journals for some time previously, inserted in yours and other periodical works, to address the gentlemen engaged in these observation, through your means (see Vol. XXX, p. 285), to suggest, from considerable series of observations by myself and others on these bodies, that their appearance at particular times was in no way influenced by the particular state of our atmosphere, (any more than the appearance of the moon at particular periods of her revolution); except only, by the absence of clouds and haziness to obscure, and of greater degrees of light from other sources to overpower them (as the stars &c. are by the day-light); and to request the minute attention of these and other meteorologists to the particular circumstances, decisive of these my suggestions being well or ill founded. Since which, and probably in consequence, the references to these phenomena, before so frequent, have nearly or altogether ceased in the Journals of Meteorological observations referred to; but nothing farther has appeared on the subject, until your last number, in which, p. 229, a very respectable and veteran meteorologist,

Shooting stars not connected with the state of the atmosphere.

Mr. De Luc's hypothesis, meteorologist, J. A De Luc, Esq., has spoken of this phenomenon as being occasioned by jets or streams of some fluids, from the surface of the Earth into the atmosphere, and the falling again of the same in a phosphorescent state, &c.

The same sincere desire for the extension of our knowledge on this very interesting subject, that induced me formerly to address the gentlemen alluded to, now prompts me to state, that the above explanation of the phenomenon by Mr. De Luc is at variance with all the best observations that I have made, or seen recorded, concerning these meteors: and that the fact of their general approach to rectilinear courses* exactly accords with all their other appearances, as being those of bodies describing parts of very large ellipses, in one of the foci of which the centre of our planet is situate; and all, except a very few of them (which from their apparent size would not be called falling stars by any one, but large meteors, as I apprehend), are moving so distant from us, and in such rare parts of our atmosphere, though with satellitic velocity, as not ever to be turned suddenly out of their courses, by highly condensed air before them, as Mr. De Luc with great appearance of probability maintains the electric spark to be deflected on or near to the surface of the Earth.

does not agree with the facts.

The train of light an optic illusion.

And farther, I beg to repeat my conviction, that all the streaks of light, that I have sometimes seen, as following shooting stars and meteors, are to be referred to the eye remaining stationary, or nearly so, during the observations that were followed by streaks of light: and I venture to recommend, as decisive of this question, that two, three, or more intelligent persons should observe in conjunction, and each one without communication with the others, write down as quickly as possible the circumstances attending his observation: and if then it happens, as it invariably has done with myself and others, either that there are no streaks seen, or that some will see streaks and others none, according as their eye does or does not adapt itself to the apparent motion of the meteor; then this supposed evidence of streams of phosphorescent fluids

* But which is not their invariable appearance, as I have sometimes seen them move in curves.

must

must be abandoned, as untenable. Hoping that the subject will ere long attract the attention of observers in earnest, and be fully elucidated,

I remain, sir,

Your obedient humble servant,

J. FAREY.

Westminster, July 2, 1812.

VII.

On Galvanic phenomena. In a Letter from J. A DE LUC, Esq, F. R. S.

To W. NICHOLSON, Esq.

SIR,

DR MAYCOCK's papers in your Journal have much interested me, as affording the opportunity of very useful disquisitions on important objects of natural philosophy; they are contained in your Numbers 131 and 144, the former of which will be the subject of my present remarks. Dr. Maycock's observations

This paper has the following title: *Observations on the hypothesis, which refers chemical affinities to the electrical energies of the particles of metals*: an hypothesis introduced by Sir H. Davy in a lecture to the Royal Society in 1807. The paper of Dr. Maycock (as mentioned in a note) had before (deservedly) obtained the gold medal of the Medical Society of Edinburgh, on this question: " Whether are the " phenomena produced in the decomposition of bodies by " *galvanism* capable of being explained by the usual princi- " ples of *chemical attraction*; or do they seem to establish " the theory, that *chemical phenomena* depend entirely on " the *electrical energies* of the particles of matter." on the reference of chemical affinity to electrical energy.

I had already, Sir, refuted this last hypothesis almost a year before, in your Journal for June 1810, by the analysis of the phenomena of the *galvanic pile*; but Dr. Maycock has more deeply treated this subject in the first two *sections* of his paper, proving irresistibly, by direct chemical experiments, that they could not be referred to *electrical energies*. And indeed Sir H. Davy himself has since expressed some doubts on his own theory. Complete refutations of the hypothesis.

But

Objection to Dr Maycock's 3d section.

But after having acquiesced with pleasure in the merit of these first two *sections* of Dr. Maycock's paper, I cannot do the same with regard to the third *section*, nor can I acquiesce in the consequences he derives from it in your No. 144. But how can it be that Dr. Maycock, though he addresses to you his papers, never mentions five of mine on the same subject, inserted in your Nos. of June, August, October, and December 1810, and January 1811, in which I have treated, from experiments, all the parts of his system? Whatever be the cause of this singular circumstance, the following discussion of the subject comparatively to his system, which I hope he will see, will certainly contribute to throw light on the most important points of *electricity* and *galvanism*.

He supposes the different electrical states of metals to be produced at their separation;

Dr. Maycock begins this subject, in the third *section* of his first paper, in the following manner: "It is an established fact, that from the *contact* and *separation* of dissimilar and insulated *metals* there is such a change in the "*electrical state* of each *metal*, that, after the *separation*, the "one is found to be *positive*, the others *negative*, in relation "to surrounding bodies": Journ. vol. XXIX, p. 25. Before I proceed in this quotation, I must state the question to be decided, in order to direct to it the experiments. Dr. Maycock supposes, that there is no *electrical effect* produced during the *contact* of the *metals*; that it takes place only at their *separation*: Whereas I shall demonstrate by a great number of experiments, that these *effects* exist only during the *contact*, and that it is owing to extraneous circumstances that any effect remains after their *separation*.

but it is caused by their contact.

Dr. Maycock's apparatus

I come now to the experiments by which Dr. Maycock thinks to establish his system. "To determine this point," he says, "in place of the small *plate* which usually remains "on my *electrometer*, I adapted a *copper plate* about 5 "inches in diameter. It is evident, that, when this apparatus is placed on a common table, the *copper plate* will "be connected with the wire of the *gold leaves*, but will be "in every other respect perfectly *insulated*; and, consequently, that, whenever a state different from that of the "surrounding bodies is produced in the *copper plate*, it will "be indicated by the *divergence* of the *gold leaves*.

and experiments with it.

"The apparatus above described being so circumstanced, "that

"that the *tinfoil* of the *electrometer* was connected with the "Earth, while the *copper plate*, the wire, and the *gold leaves*, "were insulated, I brought, by means of an insulating "handle, a *zinc plate* also of 5 inches in diameter, into *contact* "with the *copper plate* on the electrometer; there was no "visible *divergence* in the *gold leaves*. On *separating* the "*metals*, the *gold leaves* immediately *diverged*. On again "bringing them into *contact*, if the charge of the *zinc plate* "had not been removed, the *leaves* returned to their natural "position. On again *separating* the *plates*, the *divergence* "took place as before....If the charge of the *zinc plate* "had been removed after the *separation*, the second contact "did not reduce the *gold leaves* to their natural state, but "left a slight *divergence* in them; and when the plates "were again *separated*, they diverged in a greater degree "than after the preceding *separation*.—Not, however, be- "yond certain limits, which apparently varied according to "the state of the *atmosphere* as to *moisture*." This cause of anomaly is possible, but very probably some other extraneous cause interfered in Dr. Maycock's operations; else *one single* contact and separation of his plates could not have produced a sensible *divergence* of the gold leaves. This is an interesting object, which I am going to explain.

Haüy's experiments the first.

Mr. Haüy, the celebrated mathematician and experimental philosopher, is the first who has proved, that, in the contact of *zinc* and *silver* (or copper), the former became *positive* and the latter *negative*: but from the account I have had of his experiments, it required about 10 *repetitions* of the operation, applied to a condenser, to make it sensible to the *gold leaves*.

These verified by the author.

It is probable that Dr. Maycock has not had the opportunity of being acquainted with these first and original experiments, else he would have found that they contradicted the results of his own: but, sir, he might have seen in pages 261 and 262 of my paper in your Journal for August 1810, that I have repeated them with insulated plates of *zinc* and *silver* 4 inches in diameter, and verified both parts of their result. My first view was to ascertain the effect of the contact of the *two metals* on their respective *electrical states*; an effect which appeared contradictory to my ob-

 servation

servation on the *galvanic pile*, related in p. 132 of your Journal for June 1810; for the extremity of this pile actually terminated by *silver* produced the *positive* divergence in the *gold leaves connected with it*; and the other, actually terminated by *zinc*, made them diverge as *negative*; which was the reverse of what Mr. Haüy had found in his experiments. I had discovered the cause of my illusion, as explained in the same paper; but this being an important point in *galvanism*, and wishing to ascertain it for my own conviction (not in view of Dr. MAYCOCK's experiments, since I did not know them), I repeated the original experiments with the *plates* above mentioned.

Repeated contacts of the plates necessary to produce any sensible effect.

These experiments showed me first the certainty of the fundamental point, that, in their *contact*, the *zinc* plate became *positive*, and the *silver* plate *negative*. But the most important part of this experiment for my present purpose relates to the *small* effect, which *one single* operation produces on the *gold leaves*. Mr. Haüy having used *plates* as large as those with which Dr. Maycock has made his experiments, ten *repetitions* of the operation on his *condenser* were sufficient to produce a sensible *divergence* of the *gold leaves*; whereas my *plates* being only 4 inches in diameter, it required twenty *repetitions* of the alternate contacts with my *condenser*, to produce a sensible divergence of very narrow and long *gold leaves*. But besides, the experiments which follow that in my paper prove also, that a certain *number* of those *contacts* produce the same *electrical effect*, as the same *number* of *groupes* of the *two metals* remaining in *contact* with each other; to which object I shall return hereafter.

Dr. Maycock influenced by a different opinion.

The opinion, however, that *one single* operation with the two plates was sufficient to produce a sensible *electrical effect* seems to have influenced Dr. Maycock's *galvanic* system; for, in continuation of the above quoted part of his theory, he thus continues, p. 26: "The experiments, to which I "have just alluded, appear to be perfectly sufficient to "point out the fallacy of the explanation, which is very "generally received, of the *excitement* of the *galvanic pile*; "the whole of which rests on the assumption, that *dissimilar* "*metals*, while *in contact*, are in different *electrical states*, the

"the one being relatively *positive*, the other *negative*; which "has been shown to be perfectly *untenable*."

Though Dr. Maycock mentions here the *galvanic pile* invented by Volta, he does not appear to have used it in his *galvanic* experiments, for he never speaks but of the apparatus of *troughs*: but if he had seen my papers in your Journal, he would have found in them the same effects produced by the *galvanic pile*, in which the *two metals* remain in *contact* during its action. I shall therefore repeat here the most essential parts of these experiments, comparing them with his system; a comparison which, thus applied, will probably fix his attention.

Mr. De Luc's arrangement.

A plate in your Journal for June 1810 shows the construction of the *galvanic pile*, which I used in these experiments, and the paper explains my motives for that construction. My *pile* was composed of two columns, in which *ternal groups*, consisting of *zinc* and *silver* plates and *wet cloth*, were contained; but the order of the succession of the *metals* was inverse in the two columns; and thus, by means of a brass slip placed at the bottom, they were united in one *pile*, the *extremities* of which were both at the top of the apparatus, each connected with a *gold leaf* electrometer; which arrangement gave the facility of bringing the *chemical* experiments, in the *glass tubes with water*, more in sight.

His analysis of the effects of the pile.

I made the analysis of the effects of this *pile* by three different separations or *dissections* of its *ternal groups*, always composed of the *two metals* and a piece of *wet cloth*. These *separations* were produced by three small upright brass wires, forming the feet of brass *tripods*. In the first *dissection* the *ternal groups* separated by these *tripods* were the *two metals* and the *wet cloth* between them; and the effects of this *dissection* of the *pile* being the same as when it was not divided, I shall relate them first.

Experiments.

These experiments begin at p. 121 of my paper in your Journal. I first *wetted* the *cloth* with pure *water*: the *pile* produced the *divergence* of the gold leaves, and the *gasses* appeared in the *water* of the glass tubes; but not the *shock*; a very remarkable *galvanic* phenomenon, which Dr. Maycock has not considered, though it leads to the real cause of the *chemical* effects of the *pile*, namely, that a *liquid* must

separate the groups of two *metals*, in order to produce a *corrosion* on their surface. But the *corrosion* operated by pure *water* not having the effect of producing the *shock*, I undertook the series of experiments beginning by the 15th in the same paper, by which I found, that for the *shock*, the *corrosion* of the *metals* in the pile was to be produced by an *acid*. These are essential *galvanic* phenomena, which if Dr. Maycock had undertaken to explain, he would have found the deficiency of his system.

Corrosion by an acid requisite to the shock.

Separation of the electrical and chemical effects.

The second *division* of the *pile* led me to discover, that the cause of its *electrical excitement* is entirely different from that of the production of *chemical* effects. In this *dissection*, the groups of the *two metals* in *contact* were separated from each other, on one side by the *wet cloth*, and on the other by the *tripods*. In this construction, the *electrical excitement* was transmitted from group to group; for the *electrical* motions of the electroscopes at its extremities were very strong, and the *gold leaves* fell entirely when the extremities were connected together by the glass tubes with *water*; but no *chemical* effect was produced in that *water*, because the *corrosion* was not produced on the surface of both the *metals*, which is the essential cause of those effects, as it occasions a modification in the *electric fluid* itself. This is a most essential circumstance in *galvanism*; and as it is thus proved, and will be farther ascertained in the sequel, it must not be forgotten in forming theories on the effect either of the *pile*, or of the apparatus of *troughs*.

The corrosion modifies the electric fluid.

The electrical effects produced while the metals are in contact.

I return now to the question, whether the *electrical* effects produced by the association of *two* proper *metals* takes place during their *contact*, or only at their *separation*. If Dr. Maycock had read my paper in your Journal, No. 119, for August 1810, he would have seen, that the *two metals* remaining in *contact* in my *pile* produced both *electrical* and *chemical* effects; and that the *intensity* of these effects was proportional to the *number* of the groups thus connected. But he doubts whether the *course* of the *electric fluid* can be determined in the *pile* and its *circuit*; and he does not even think it necessary to inquire into the cause of the *electrical* effects, for he says in p. 14 and 15 of his paper in No. 131:

Dr. M. supposes the seat of

"In the theory of Dr. Franklin, an *electric fluid* is supposed "to

"to be accumulated in the *glass*, and dissipated in the *seal-ing-wax*. Admitting the existence of an *electric fluid*, it "would seem to follow, that if it be accumulated in the *glass*, "it must be dissipated in the *sealing-wax*: but as far as my "knowledge goes, it has never been determined, that it is "in the glass, and not in the *sealing-wax*, that the accumu-"lation takes place." electricity not determined,

This opinion has led Dr. Maycock to suppose, that it cannot be determined whether a *fluid* does *enter* at a determined extremity of the *pile*, and *return* to the opposite extremity, when a *circuit* is established; but if he had seen my paper in your Journal for June 1810, he would have known, that I had made this point the object of a long series of experiments, demonstrating that, when the extremities of the *pile* are connected together by a *conducting* substance, the effects are produced by the circulation of the *electric fluid* entering the extremity, which, in the *pile* without a *circuit*, is *positive*, and returning to the extremity, which, in the same case, is *negative*. and consequently its course in the pile not ascertained.

I come to another proposition of Dr. Maycock, p. 27 of the same Number of your Journal, relating to *galvanism*. "The *galvanic apparatus*," he says, "can only be *excited* "by a *decomposable fluid*, and this *fluid* is always *decomposed* "when the apparatus is *excited*." Sir H. Davy was at first of the same opinion; but I have demonstrated by various experiments in your Journal, that no *fluid* (or *liquid*) is necessary to produce that *excitation* with respect to the *electrical* phenomena of the pile; that the only condition of this effect, distinct from the *chemical* effects, is, that the groups of *two metals* in actual *contact* be separated by a *conducting* substance not *metallic*; and as it is a very essential point in *galvanism*, I shall briefly repeat its proofs. Dr. M. supposes a decomposable fluid necessary to the excitement: but the contrary has been shown.

This, first, is the cause why, in the second *dissection* of the pile above mentioned, where that condition only existed, the *electrical* phenomena continued; but not the *chemical* phenomena, which require, as I have proved, a *liquid* between the *metals*, in order to produce a *corrosion* on their surface. This is demonstrated by Exp. 19 in p. 135 of your Journal for June 1810. For, when before I had mounted a *pile* of 76 groups composed of *zinc* and *silver* plates 1·6 inch in diameter, Experiment proving this.

in *contact* with each other, separated by pieces of *wet* cloth, that *pile*, beside the motions of the *gold leaves* at its extremities when free, produced *chemical* effects in the *water* of the *glass tubes* when forming the *circuit*; but, by substituting for the wet pieces of *cloth* pieces of the same *cloth*, new, and without *wetting* them, I had the same motions of the *gold leaves* at its extremities, only less, and not the smallest appearance of *chemical* effect in the *water* of the *glass tubes*.

Search for a conductor not metallic.

This first observation opened a new field to my view. Having conjectured, that *wool*, the material of the cloth I used, had in itself very little *conducting* faculty, and that it became a good *conductor* only by being *wet*, I undertook a long series of experiments for finding out what substance, not *metallic*, was the best *conductor*. I found in general, that *vegetable* substances were better *conductors* than *animal* ones; and among the former, plain writing *paper* having produced as much effect as any other, I fixed upon it, as being easily cut to the size of the metallic plates.

Vegetable best.

Experiment with this.

I then made the Exp. 20, which concludes the paper in your Journal. I mounted again the pile of 76 groups of *zinc* and *silver plates* in *contact*, separated only by pieces of *paper*. This *pile* produced as great *electrical* signs at its extremities, as that with the *wet cloth*, but not the smallest *chemical* effect appeared in the *water* of the *tubes*, when they connected these extremities. Thus were demonstrated the conclusions, which I had deduced from the phenomena of the *pile* itself.

Steps to which this discovery led.

My paper in your Journal for August 1810 describes the steps, to which this first discovery led me; which progress, had Dr. Maycock known it, would undoubtedly have struck him, as bringing to view an absolutely new field in experimental philosophy, not only by ascertaining the distinct causes of *electrical* and *chemical* effects in the *pile* (as indicated by the preceding experiment); but by this important phenomenon, that the *motions* of the gold leaves are very different at different times, without any connection with the difference of either *heat* or *moisture*; which changes were to be attributed to changes in the *electrical* state of the *ambient air*, from the following facts, leading to *meteorology*.

We

We have a method of comparing the *electrical* state of the *stratum* of *air* near the *ground* with that of the *strata* higher up, in which we can elevate a *conductor*; the comparative point of which, or the standard of *positive* and *negative*, is the *electrical* state of the ground; whereas, we have no such point of comparison within the *stratum* near the *ground*, in which our experiments are made; as the latter influences too much the state of the air near it; however I have observed changes in the *electrical* state of this lower *stratum* in the following manner. Having employed columns of many hundred groups, the *gold leaves* not only *diverged* very strongly, but they struck the tinfoils, fell, as being discharged, then rose and struck again. Now, the *number* of these alternate motions, in a given time, differ so much in different days, that I have seen sometimes 60 strikings in a minute, while at other times there was not even one in the same interval. This, as I have said and constantly observed, not having any connection with the changes of either *heat* or *moisture*, depends very probably on changes in the *electrical* state of the *ambient air*.

Electricity of the air near the ground and higher up may be compared.

Changes in the lower stratum also discoverable.

But let us fix our attention on the motions of the *gold leaves* in the electrometer, in order to understand the influence of the state of the *air* on the apparatus. On this essential point, Sig. Volta has made a very important step in electricity, which has removed the difficulties, till then insurmountable, in Dr. Franklin's system of *positive* and *negative*, without reference to any known *standard*. But Sig. Volta has first proved, that the particles of *air* possess the electric fluid as well as other bodies; and that the *electrical* state of the air in the place of observation is the *standard* of *positive* and *negative* with respect to the *electrometer*. Therefore, the *motions* of *gold leaves* indicate only the actual *electrical* state of the *air* which environs the instrument. It is impossible however to follow, in its phenomena, all the effects of the *electric fluid*, without a determination of its *nature*, which Dr. Maycock considers as unnecessary. I have given that determination in the same paper above mentioned, beginning at p. 254; but the phenomena on which it is founded are so numerous, that even in that paper I could only give a short account of them, referring to my works

Standard of positive and negative electricity discovered by Volta.

works, *Idées sur la Météorologie*, and *Traité élémentaire sur le Fluide électro-galvanique*. In these works, I have demonstrated, by a long series of experiments, what I shall now summarily state on this subject.

The electric fluid a compound.

The *electric fluid* is composed of many *ingredients*, which however are only manifested when it exhibits *sparks*, by darting from one *conductor* to another. At this instant three phenomena are observed, which the *electric fluid* does not produce when it only moves along conductors; they are *light*, heat, and a peculiar *smell*. These sudden phenomena must be produced by a *decomposition* of the *electric fluid*; and in following the other phenomena attending this *decomposition* I have shown, that, beside the three *ingredients* thus manifested, *light*, *fire*, and an *odorate substance*, there are other *ingredients* in the *electric fluid*; one of which, well determined, is a most *tenuous fluid*, which imparts its strong expansibility to the others, and is the cause of the phenomenon called *electric influences*; a most characteristic effect of the *electric fluid*, which I have followed by exact experiments, beginning at p. 267 of the same paper. The *fluid* thus manifested was, for the facility of expression, to have a name; and I have called it *vector*, as giving *motion* to the *unexpansive* substance, which constitutes the *density*. Now, in the course of these experiments I have demonstrated, that *electrical motions* are produced only by the substance constituting the *density*, without any participation of the *fluid* producing the *electric influences*. This is an indisputable proof, that the *electric fluid* is a *compound* substance.

Similar effects from electricity and the pile.

There was another point to be determined in galvanism, which Dr. Maycock not having considered, his system remains without any foundation. The same effects as from the *galvanic pile*, namely the *shock* and the *gasses* in *water*, are produced by the *electric machine* charging a *battery* of *coated jars*; but in this case they are produced by a very much *condensed* electric fluid; while, when the same *fluid* has pervaded the *galvanic pile*, it produces these effects with an incomparably smaller quantity. With regard to this subject, I have proved by many experiments, what is above stated, that the *chemical* effects produced by the *pile* proceed

proceed from the *electric fluid* pervading it during the *corrosion* of the *metals* effected by a *liquid*; an alteration by which this *fluid*, though with a very small *density*, is *decomposed* when passing from one *conductor* to another. This important point, both in *electricity* and *galvanism*, is proved by a series of experiments beginning at p. 243 of the same paper.

Effect of pasting paper on one of the metals.

Having been led by these experiments to the above-mentioned apparatus, wherein the groups of the *two metals* were separated by *writing paper*, it came into my mind to try, whether there would be any advantage for increasing the *electrical* effect of this new kind of *pile*, to fix, by pasting, that *paper* on one of the *metals*. I made this experiment on *zinc*, and *copper* or *silver*, the former of which becomes *positive* and the two other *negative* while in *contact*, and on *pewter*, which in contact with *zinc* becomes *negative*, and *positive* with *silver* or *copper*. An additional proof, that there is neither *positive* nor *negative* state belonging to any kind of body. The general result of these experiments, detailed in p. 245 of my paper, was, that there is a sensible increase of effect by pasting the *paper* on that *metal*, which in contact with the other becomes *negative* by losing some of its *electric* fluid and yielding it to the other; such as *silver* and *copper* with *zinc*.

Construction to which this led.

This opening the prospect of obtaining a spontaneous and permanent *electric machine*, the power of which might be increased almost without limit by increasing the number of groups, I was going to undertake it in some measure, by pasting *paper* over copper plates of the same size of my *zinc* plates; when luckily it occurred to my recollection, that there was *paper* on which *copper* was ready laid, called *Dutch-gilt paper*. The experiment 27, p. 246 of my above-mentioned paper, relates my first trial. I constructed one of these new *piles* consisting of seventy-six groups of the same *zinc-plates* of 1·6 inch diameter, separated by equal pieces of *Dutch-gilt paper*, all the *copper* sides of which were turned towards the same extremity of the column. Thus I had seventy-six groups of *zinc* and *copper* in mutual *contact*, separated by the *paper* on which the *copper* was laid. Now, these seventy-six groups produced greater *electrical* effects

at

at their extremities, than the same number of groups of *zinc* and *silver* separated by the *wet cloth*: however now not the smallest *chemical* effect was produced in the *water* of the *glass tubes* when connecting these extremities, though the *gold leaves* fell; a proof of the *circulation* of the *electric fluid.*

In p. 250 of the same paper, at exp. 29, begin some trials concerning the comparative *electrical* effects of the *size* and *number* of the *groups*, on which I had already formed my judgment, by Sig. Volta having explained to me at Paris, in 1782, the cause of the effect of his admirable instrument then lately invented, the *condenser*, as explained in that paper; according to which I found, that, for a mere *divergence* of the *gold leaves*, the *number* only of the *groups* determined it: but that, when they produced some other effect; as for instance to strike the sides, and thus be reduced to the *electrical* state of the *ground*; with the same *number* of *groups*, the *size* of the *plates* had an influence, as they repaired sooner what the *gold leaves* had lost; which made them strike more frequently in the same time, in proportion to the *size* of the *plates*.

Volta's condenser.

Different effects of size and number.

I come now to the general results of these experiments, the account of which begins at p. 262 of the same paper; which will show what I have above mentioned, with respect to Dr. Maycock's system, that a certain number of groups, *zinc* and *copper* being in mutual *contact*, and separated by *paper*, produce sensibly the same *electrical* effect, as the same *number* of *contacts* of one insulated *metal*, after having been applied to the other while communicating with the *ground*. I made these experiments with a particular *condenser*, shortly described in my paper, and with plates of the same diameter as those of my column. I have given the general results of these experiments in some *tables*, p. 265, in all of which, A represents the *zinc* side, and B the *copper* side. In these *tables*, designed to trace the motion of the *electric fluid* through the *pile* in different circumstances, I have supposed the *pile* of the new construction, (which I have called *electric column*) of whatever number of *groups*, to be divided into eleven equal parts. I have used arbitrary *numbers* to express the *progress* of *positive* and *negative*, but they are proportional to the whole: I have made these *numbers*

Number of pairs of metals produce the effect of the same number of contacts of one pair.

Results tabulated.

bers increase regularly, though regularity cannot be expected in such experiments: but these *numbers* do not differ essentially from the immediate results. Results tabulated.

The first two columns, in that p. 265, represent the simultaneous progresses of *positive* and of *negative* in the *insulated* pile; one expressing the progress of the *negative* from A to B, and the other, the progress of the *positive* from B to A. These are as the elements of the combination of effects shown in the three following *tables*, in three different situations of the *pile*.

Table I.	Table II.	Table III.
Insulated pile.	B in communication with the ground.	A in communication with the ground.
A	A	A
+ 10	+ 20	0
+ 8	+ 18	— 2
+ 6	+ 16	— 4
+ 4	+ 14	— 6
+ 2	+ 12	— 8
0	+ 10	— 10
— 2	+ 8	— 12
— 4	+ 6	— 14
— 6	+ 4	— 16
— 8	+ 2	— 18
— 10	0	— 20
B	B	B

I cannot think, that Dr. Maycock has seen these experiments; for had he doubted their results at first, he would have found them confirmed in my following paper, with such evidence, that he could not have avoided, either to disprove them, or to show that they were not against his system.

I shall not enter into an account of all the experiments contained in this paper, as it would be a repetition in the same Journal. I had only here in view Dr. Maycock's system, which, according to my judgment, involves in obscurity the whole field of electricity and galvanism; I was therefore to recall those only of these experiments which relate to this subject: but they are more, sir, to this purpose, in my

my paper in your following number for October 1810, which I shall also recall for the same motive in a following communication, and I hope that the whole together will induce Dr. Maycock to change his system.

I am, with great regard, sir,

Your most obedient servant,

Windsor, J. A. DE LUC.

July the 10th, 1812.

VIII.

Explanation of a hydrostatical Phenomenon observed by Franklin: *by* Robinet*.

Agitation of water underneath oil.

If you put water and oil into a tumbler, suspend the tumbler by a string, and give it a gentle swing, you will perceive nothing particular at the surface of the oil; but the surface of the water beneath will appear agitated, and form considerable waves. Such was the phenomenon observed by Franklin, and with which he was puzzled no doubt merely because he had not time to examine it: for that great man had so acute an eye in observing nature, that he scarcely ever failed to seize those connexions of facts, that constitute properly what we call physical laws. His confession of his ignorance on this occasion merely proves, that his modesty knew how to avail itself of his being too much occupied to examine every thing.

The phenomenon capable of variation.

This phenomenon is capable of assuming very different forms, by varying the circumstances by which it is produced. I shall confine myself here to that observed by Franklin, because it is one of the most complicated.

The facts result from two known principles.

The facts that compose this phenomenon, some of which are known to all the world, are the result of two hydrostatical principles, well known separately, but not yet considered together; that by which liquids seek their level, and that with the discovery of which Archimedes was so delighted.

Tendency of a fluid to a level.

By the first, all the parts of a liquid equally heavy, and perfectly movable on each other, tend toward the centre of the

* Abridged from the Journ. de Phys., vol. LXV, p. 277.

the Earth with equal energy; and, approaching it as long as they find no sufficient obstacle in the adjacent columns, do not stop till they arrive at that state, which is called the level. And as this motion is an effect of gravity, it is accelerated, carries all the parts beyond the point of equilibrium, and causes them to vibrate several times round this point, producing undulations, a kind of oscillations with which every one is familiar.

Motion of a body in a fluid.

By the second principle, a body moving in a liquid, being obliged, in displacing it, to communicate to it continually a part of its motion, is incessantly losing force; so that, in obeying the laws of gravitation, it falls through the liquid only with the excess of its specific gravity over that of the liquid in which it moves.

Application of these principles.

It might be supposed at first view, that this second principle should have no influence over either of the two liquids, that exhibit the phenomenon in question, because neither of them is properly in the other. Both, however, are subjected to this law: the lowermost, because its surface cannot acquire any undulatory movement without displacing the upper; and the uppermost, because its surface cannot move without raising the air, which then presses on it at every point. But, as we are accustomed to see the effects of this position with regard to the air, we do not think of referring them to their cause.

With respect to the inferior liquid, its situation and relation to the superior render a phenomenon very remarkable, which at the same time is essentially the same with what we see without attention at the surface of the superior.

Change of level in a fluid beneath another not much lighter.

To understand the reason of this singularity, let us suppose some cause to have disturbed the surface of the inferior liquid, so that it is no longer level, but a given column is a certain degree higher than another. This column does not exceed the shorter in weight by the whole quantity it is higher, for it makes only a part of the column that exists in the vessel at that point, consisting of the heavier liquid at its lower part, and the lighter at its upper. The shorter column of the inferior liquid in like manner is only the lower part of a column, the upper part of which is formed of the lighter liquid. The difference between these two columns is,

the

the first has more of the heavier liquid and less of the lighter, the second more of the lighter and less of the heavier.

Conditions necessary to its restoration.

For these two columns to acquire their level it is necessary, that the first should lose a portion of the heavier liquid and acquire a portion of the lighter, and the second the contrary. And as there is no cause to produce this effect but the portion of the heavier liquid that one column has more than the other, the reduction of the inferior liquid to a level cannot be effected by the absolute gravity of the liquid as happens when it is alone in a vessel, but must be caused only by the excess of the weight of the inferior liquid over that of the superior.

The equilibrium more slowly restored,

Hence it follows in the first place, that, as the restoration of the level of the inferior liquid is the effect of a very small part of its gravity only, it must be extremely slow, and in consequence capable of being observed more easily, than when this liquid is alone in the vessel. It may not be amiss to observe however, that this cause, however small it may be, being a gravitating action, must retain its nature of an accelerating force, and thus produce an undulatory motion as in ordinary circumstances.

and its disturbance more conspicuous.

If now we attend to the interruption of equilibrium, or of the level, between the several columns of the inferior liquid, we shall find, that the same cause, which renders the restoration of the equilibrium slower and more obvious, renders its interruption likewise more considerable.

This scarcely observed with a single liquid.

Gravitation, as it exists before our eyes, imparts to ordinary bodies, in the shortest space of time we can estimate, a velocity very similar to those which we ourselves very commonly produce: so that when we do any thing to disturb the level of a liquid surface it is restored almost immediately. If we give a moderate inclination to a vessel filled with a liquid, the level is restored in proportion as we endeavour to destroy it; so that it requires some little knowledge of natural physiology to be aware, that it has been disturbed and restored.

Case of two liquids

But in the circumstances in which the inferior liquor is placed; as but a small portion of its gravity remains to reduce it to a level, it is evident, that it cannot effect this with the same promptitude; and that, if the same motion be employed

ployed to disturb its equilibrium, this will take place to a considerable degree, in a time when it would have been scarcely perceptible had there been but one liquid in the vessel. Thus, in the instance stated by Franklin, the swinging of the glass produces scarcely any agitation at the surface of the oil; because, though the glass inclines alternately to each side, as the motion is moderate, the surface of the oil returns to an equilibrium as fast as it is diverted from it. But the surface of the water, having to restore its equilibrium only the excess of its weight over that of the oil, which is very trifling, as we may estimate it at about 0·006 of the weight of the water, allows the small deviations from the level time to accumulate; so that this surface is no longer level when the swinging ceases, and is obliged to return to it by very slow and considerable undulations, that continue a long time. as observed by Franklin.

Of all the modes, in which this phenomenon may be varied, I shall mention but one, where the cause I have mentioned is too obvious to be mistaken. The experiment varied.

Take a glass globe, mounted so as to be capable of being turned on its axis; put into it water alone, so as to fill it to a quarter of its diameter; and turn it gently: the water will continue apparently to occupy the lower part of the globe. Fill it then to three fourths, and the appearance will be the same, if you turn it in the same manner. So it will if you put oil alone, instead of water. Lastly, pour in water to one fourth of its height, and upon this oil to three fourths: then, if you turn the globe, there will be no change at the surface of the oil, and the whole body of liquid will appear to occupy the lower part of the globe. But with the water it will be different. When you have turned the globe a quarter round, you will perceive it nearly at the extremity of the horizontal diameter, instead of being in the lower part of the globe: and, if you then stop the rotary motion, the water will descend slowly down the side of the globe to the lower part, will ascend on the other side nearly to the same height, and thus oscillate a long time, till it settles at its lower part.

As I have said above we here see clearly, that the particular motion of the water under the oil has the particular character of that of solid bodies in fluids: and as it is the phenomenon

nomenon of Franklin divested of all accessory complication, there can remain no doubt of its true cause.

This theory practically applied in common life.

They who frequently carry liquids in open vessels had at least an obscure perception of this theory. They know by experience, that the liquid is much less liable to be spilt by sudden movements, if a light body float on their surface. For this reason water-carriers put a wooden trencher into each of their buckets; and in vineyards a broom is put on the wine, when it is carried from the press to the cellar in an open wooden vessel. Any motion begun or terminated too suddenly would produce a considerable change of level in these liquids, a wave that would cause them to overflow. This wave is nearly prevented by the existence of the light body, that swims on the liquid; because all the columns, that terminate in this body, find in it an obstacle to their undulatory motion, as they can rise or fall only with this body itself: and as it corresponds to a great number of columns at the same time, and is urged in opposite directions by different columns, it is a considerable obstacle to them all; and thus it influences those columns it does not cover, since these cannot undulate separately from the others.

IX.

On the Nature of Sheep's Dung, and its use in dyeing Cotton the Red that is called India or Adrianople: by J. B. VITALIS, *Professor of Chemistry at Rouen**.

Process for dyeing cotton red.

THE process followed at present in our manufactories for dyeing cotton red, and which was first brought from the Levant, is composed of a series of operations, each of which requires to be elucidated by chemistry, if we would be certain of obtaining uniform success in this sort of dyeing.

Chemical examination of it.

Employed by government to teach the principles of chemistry in all its connexions with the useful arts, I thought it my duty to pay particular attention to that branch of industry

* Abridged from the Journ. de Phys. vol. LXVI, p. 153.

industry, which constitutes the base of the employment and trade of the first manufacturing city of the French empire.

Dyers of Rouen.

The manufacturers of Rouen employ both fast and false colours. I have already imparted to the latter, by the help of certain mordants, a degree of richness, lustre, and even permanence, before unknown; which no doubt procured the specimens I sent the honour of being admitted to the exhibition of 1806.

Articles of inferior price.

The class of citizens who are not wealthy, and they are the most numerous, require clothes of a price proportionate to their circumstances. Besides, the dyeing of inferior colours employs a number of workmen, and yields a profit, that would soon be seized by other towns, if it were despised here.

The best dyes most important,

and carried to great perfection.

But the reputation and wealth of our manufactories are derived chiefly from the colours called fast, that is to say those that are produced by the process for Adrianople red. These colours have opened a vast field of inexhaustible fertility to the manufacturer. He can now employ in his designs that variety, that happy mixture, that elegant association, that harmony of colours, which are so pleasing to the eye, and so gratifying to the taste of the most fastidious. Instead of those perishable colours, that delighted for a moment, the Indian red, and the extensive series of colours derived from it, as the cherry, rose, violet, lilac, julyflower, amaranth, &c., in all their various tints, have little to fear from the most destructive agents, and scarcely yield to the long continued action of air, light, and soap.

Some manufacturers fail in them.

This process therefore is of the highest importance to us; but, though it is practised with the greatest success by some manufacturers, others meet with obstacles, that occasion failures, which it would be highly useful to be able to prevent. I have endeavoured as far as possible to remove these, and to dissipate the uncertainty attending the operations performed on the cotton intended to receive this colour, by a chemical investigation of them.

Use of sheep's dung in dyeing red.

I have the honour now to present to the Institute the result of my examination into the nature and use of sheep's dung in dyeing Adrianople red, my object being to impart solid

solid notions of the mode of action and influence of the sheep's dung bath, the first applied to the cotton.

Mistaken opinion of it.

Various opinions have been broached on this subject; but the experiment, of which I am about to give an account, will at least dissipate every idea of its containing a large quantity of volatile alkali, to which La Pileur d'Apligny ascribes its property of *rosing* reds.

Sheep's dung distilled.

In May 1806 I distilled 61·19 gr. [945 grs] of fresh sheep's dung in a coated glass retort, to which I fitted a receiver furnished with a tube of safety, and a tube for collecting the gaseous products. The retort was placed in a reverberatory furnace, and gradually heated till the bottom was red.

Results.

On receiving the first impression of the fire, a very clear liquid passed over. On raising the heat, white vapours were evolved, oily, not very copious; and soon succeeded by drops of a very fluid oil, the colour of which was a very fine orange yellow. To this oil succeeded a second, thick, almost concrete, of a blackish brown, and smelling strongly empyreumatic. During the distillation about 50 cubic inches of elastic fluids passed over, which were found to be a mixture of carburetted hidrogen and carbonic acid.

Residuum.

Having broken the retort, I observed, that it was lined interiorly with a slight coating of coal, exhibiting the metallic lustre, and assuming on exposure to the air, though only in some places, the blue colour of prussiate of iron. At the bottom I found a dull black coal, tolerably dense, retaining the shape of the matter subjected to analysis, without any sensible taste, and exhaling a smell precisely like that of tobacco smoke.

This coal weighed 7·8 gram. Heated in a porcelain crucible it readily took fire, and before the vessel was redhot. I observed, that it emitted oily and empyreumatic vapours, owing no doubt to a small quantity of oil, with which it was still impregnated; and that it burned with a small white flame. After burning six hours with a fire well kept up, it left 3·68 gr. of a gray substance, which was found to be phosphate of lime.

Oils.

Of the two oils mentioned above I collected 3·91 gr.

Phlegm.

The coloured liquor in the receiver, contaminated with a few

few drops of fluid oil, weighed 48·8 gr. It turned sirup of violets green; at the same that it reddened infusion of litmus, though it is true but faintly.

The last-mentioned property was owing to a small portion of acetic acid, which was formed in the course of the distillation: I think its changing sirup of violets green may be ascribed to the presence of a small portion of gelatinous matter, that had passed over with the aqueous vapour, by which it was held in solution.

For the rest, on assaying the liquor by every known method, no test discovered in it the least trace of ammonia. No ammonia.

From this experiment it appears, that 61·19 gr. of fresh sheep's dung yielded by distillation

Products.

An acid and alkaline liquor	48·80
Gaseous fluids	0·58
Concrete and fluid oil	3.91
Charcoal and phosphate of lime....	7·80
	61·09
Loss	0·1
	61·19

From these results I think I may conclude, that sheep's dung contains much more hidrogen than nitrogen, which appears to me demonstrated, 1st, by the great quantity of water furnished by the matter analysed, and which certainly did not exist in it ready formed: 2dly, by the hidrogen gas collected under the jar: 3dly, by the oil obtained: and 4thly, by the absence of ammonia during the whole of the process. The dung contains more hidrogen than nitrogen.

It appears to me therefore proved, not only that ammonia does not exist in sheep's dung, but that it cannot be formed in it in large quantity.

But let us go farther, and suppose for a moment, that sheep's dung contains a certain quantity of ammonia; is it not evident to all, who are acquainted with the process for Adrianople red, that this alkali, so volatile in its nature, could Ammonia could not have the effect ascribed to it.

could not undergo the numerous manipulations and repeated dryings, either in the open air or by the heat of a stove, to which the cotton is subjected, without being entirely disengaged? Were it to be urged, that the alkali is rendered fixed by combining with the cotton; I should require proof of this, the contrary of which is shown by experiment.

Composition for brightening red.

But the property thus ascribed to ammonia of *rosing* cotton, that is of brightening the tint of madder red, and imparting to it warmth, lustre, and liveliness, is equally unfounded; for these effects can be produced only by forming with white marseilles soap and muriate of tin a metallic soap, in which the oxide of tin is held in solution by soda.

Sheep's dung useful by its albumen and gelatine.

Thus, since neither does ammonia possess the properties ascribed to it, nor is it contained in sheep's dung, we must look for the cause of its effects in some other principle. Now this can be nothing but the albumino-gelatinous matter so abundantly contained in sheep's dung: to convince ourselves of which, we have only to attend to the manner, in which it is used.

Mode of employing it.

In the first place the dung is macerated in a solution of soda, of the strength of about 4° [sp. gr. 1·027], for some time. The effect of this maceration is evidently the solution of the albumen and gelatine by means of the alkali. A certain quantity of this solution, passed through a sieve and diluted with a solution of soda at 2° [sp. gr. 1·013], is mixed with thick or mucilaginous olive oil; and thus a kind of liquid animal soap is formed, with which the cotton is carefully impregnated.

This impregnates the cotton with animal matter.

In this process the cotton, by combining with the albumen and gelatine, approximates to the nature of animal substances; which, as is well known, have a stronger attraction than vegetable substances for colouring matter. The combination appears to be farther promoted by the oily principle, that combines with the cotton at the same time.

Intestinal fluid of the sheep equally useful.

We now see why authors, who have written on India red, recommend the use not only of the dung, but also of the intestinal liquor of the sheep; which it would be much more advantageous to employ, were it possible to procure it in sufficient quantity for the demand.

The

The theory just laid down is supported by experiment. Having macerated fresh sheep's dung for four or five days in a lixivium of soda at 4°, I filtered, and obtained a reddish brown liquid. On separating the alkali by very dilute sulphuric acid, a copious, light precipitate was formed, which subsided to the bottom of the vessel, after having for some time occupied its whole capacity.

Sheep's dung macerated with alkali yielded a precipitate,

To remove all doubt respecting the nature of this precipitate, I collected it on a filter, washed it well with cold water, and then boiled it in a phial of pure water for near an hour. I then decanted off the liquid, which was of a reddish yellow, and poured into it a solution of tannin. This formed a precipitate, announcing sufficiently the presence of gelatine.

consisting of gelatine

The albumen, coagulated by the action of the heat, remained at the bottom of the phial in the form of little soft and spongy grumes: and to judge by the quantity of matter insoluble in water, though it was renewed three or four times, albumen abounds much more than gelatine in sheep's dung. I do not think it would be far from the truth to say, that the albumen is to the gelatine at least as three to one. Particular circumstances prevented my carrying the investigation to such a degree of accuracy, as I could have wished for my own satisfaction.

and albumen.

The latter most abundant.

To establish a complete conviction on this subject, I shall add, that I tried an alkaline solution of whites of eggs, or albumen, instead of the sheep's dung bath; and that it succeeded completely in the preparation for both kinds of dyeing: all the colours were rendered much more permanent, than where natural or artificial sheep's dung baths were omitted.

An alkaline solution of albumen answered equally well.

This observation, founded on theory and experience, completely refutes the assertion of Le Pileur d'Apligny, that the dung and intestinal liquor of the sheep are of no use in fixing colours.

X.

X.

METEOROLOGICAL JOURNAL.

1812.	Wind	Pressure. Max.	Min.	Med.	Temperature. Max	Min.	Med.	Evap.	Rain	
6th Mo										
June 2		29·98	29·95	29·965	68	53	60·5			☽
3	N E	30·02	29·98	30·000	65	46	55·5			
4		30·04	30·00	30·020	72	50	61·0	·38		
5	E	30·08	30·04	30·060	70	49	59·5			
6	N E	30·18	30·08	30·130	66	45	55·5			
7	N E	30·35	30·12	30·235	70	44	57·0	·52		
8	N E	30·40	30·35	30·375	62	46	54·0			
9	N	30·40	30·15	30·275	66	51	58·5			●
10	Var.	30·27	30·17	30·220	65	43	54·0	·36		
11	N W	30·07	30·03	30·050	75	53	64·0			
12	N W	30·03	29·93	29·980	74	48	61·0	·33		
13	S W	29·93	29·88	29·905	68	50	59·0			
14	S W	29·88	29·81	29·845	69	49	59·0			
15	S W	29·82	29·79	29·805	68	49	58.5		·07	
16	S W	29·79	29·58	29·685	65	48	56·5		·23	☾
17	S W	29·78	29·58	29·680	52	46	49·0		·39	
18	S W	29·58	29·49	29·535	59	53	56·0		·40	
19	S W	29·49	29·34	29·415	63	49	56·0	1·15	·09	
20	S W	29·53	29·33	29·430	60	46	53·0		·21	
21	S W	29·66	29·32	29·490	60	46	53·0		·16	
22	S W	29·83	29·66	29·745	60	43	51·5	·37	·01	
23	W	29·94	29·81	29·875	62	46	54·0		·05	
24	S W	29·94	29·91	29·925	59	45	52·0		·06	○
25	Var.	29·91	29·60	29·755	63	50	56·5		·25	
26	Var.	29·86	29·45	29·655	58	42	50·0	·38	·61	
27	Var.	29·86	29·78	29·820	63	46	54·5		·22	
28	N	30·10	29·78	29·900	58	39	48·5		·01	
29	S W	30·03	29·96	29·995	64	48	56·0			
30	S W	29·86	29·70	29·780	62	52	57·0	·60	·05	
		30·40	29·32	29·881	75	39	55·87	4·09	2·81	

The observations in each line of the Table apply to a period of twenty-four hours beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

NOTES.

NOTES.

Sixth Month. 3. A little rain at intervals. 4. A few large drops: cumulo stratus p.m. A shower to the S.W. Wind E. 6. Much dew: clear with cirrus. 6. Overcast, windy: then very fine with red cirri at sunset. 7. Cloudy morning: clear day afterward: brilliant orange twilight. 8. Cloudy: brisk wind. 9. Fair, with cumulus, and cirrus above: at sunset the wind rose, with some appearance of nimbus. 10. Cumulo-stratus, with a cold breeze all day. 11. A. m. wind fresh at W.: the maximum of temperature occured at nine: the barometer fluctuating. Cumulus clouds, with very large plumose cirri above, which showed red at sunset. The new moon appeared (in a white crescent, becoming afterward of a gold colour) in the midst of a pretty luminous twilight. 12. A.m. cloudy: barometer still unsettled: evening twilight luminous and orange coloured: a *stratus* began to appear at nine p. m. 13. A. m. misty: much dew. 15. Cool day: rather windy. 16. Rain last night: fair and cool. 17. Heavy short showers. 18. Fair, cloudy: rain by night. 19. The rainbow *twice* this morning. 21. Several hours' rain a. m. Barometer fluctuating. 22. Nimbi a. m. fair p. m. 23. Nimbi through the day: thunder twice to the S.W.: the wind veered as far as to N. W. but settled W. 24. A. m. much cloud: calm air: showers. 25. Cumulus, with very elevated cirrus in parallel bands E. and W. A *solar halo* for above two hours soon after noon, the higher atmosphere filled with cloud: at sunset the wind, which had been S. E. and S. W., came to N. W. 26. Cold stormy morning, wind N. Thunder twice about two p. m.: rain almost from sunrise to sunset. 27. A. m. sunshine: wind N. W.: a *solar halo* p. m. wind S. W.: evening wet and stormy. 28. A. m. wind N. a faint blush on the evening twilight. 30. Windy evening: rain at intervals.

RESULTS.

Winds variable, the South-west most continuous.

Barometer: highest observation 30·40 inches; lowest 29·32 inches; Mean of the period 29·881 inches.

Thermometer: highest observation 75°; lowest 39°; Mean of the period 55·97°.

Evaporation 4·09 inches. Rain 2·81 inches.

This period is remarkable for being pretty equally divided into a dry and a wet moiety: the former commencing with the first quarter, the latter the day before the last quarter of the moon. The return of the first quarter appears (by subsequent observations) to have again nearly coincided with that of dry weather.

PLAISTOW. L. HOWARD.
Seventh Month, 15, 1812.

XL.

XI.

An Account of "The Sulphur," or "Souffrière" of the Island of Montserrat: by NICHOLAS NUGENT, M. D. *Hon. Member of the Geological Society*.*

Occasion of the visit.

ON my voyage last year (October 1810) from Antigua to England the packet touched at Montserrat, and my curiosity having been excited by the accounts I received of a place in the island called "The Sulphur," and which, from the descriptions of several persons, I conceived might be the crater of an inconsiderable volcano, I determined to avail myself of the stay of the packet to visit that place.

Face of the island.

The island of Montserrat, so called by the Spaniards from a fancied resemblance to the celebrated mountain of Catalonia, is every where extremely rugged and mountainous, and the only roads, except in one direction, are narrow bridle paths winding through the recesses of the mountains; there is hardly a possibility of using wheeled carriages, and the produce of the estates is brought to the place of shipment on the backs of mules. Accompanied by a friend, I accordingly set out on horseback from the town of Plymouth, which is situate at the foot of the mountains on the seashore. We proceeded by a circuitous and steep route about six miles, gradually ascending the mountain, which consisted entirely of a uniform porphyritic rock, broken every where into fragments and large blocks, and which in many places was so denuded of soil, as to render it a matter of astonishment how vegetation, and particularly that of the cane, should thrive so well. The far greater part of the whole island is made up of this porphyry, which by some systematics would be considered as referrible to the newest floetztrap formation, and by others would be regarded only as a variety of lava. It is a compact and highly indurated argillaceous rock of a gray colour, replete with large and perfect crystals of white feldspar and black hornblende. Rocks of this description

Journey to the Sulphur.

* Trans. of the Geol. Soc. vol. I, p. 185.

generally

generally pass in the West Indies by the vague denomination of fire stone, from the useful property they possess of resisting the operation of intense heat. A considerable quantity of this stone is accordingly exported from Montserrat to the other islands which do not contain it, being essential in forming the masonry around the copper boilers in sugar works. We continued our ride a considerable distance beyond the estate called "*Galloway's*," (where we procured a guide) till we came to the side of a very deep ravine which extends in a winding direction the whole way from one of the higher mountains to the sea. A rugged horse-path was traced along the brink of the ravine, which we followed amidst the most beautiful and romantic scenery. At the head of this ravine is a small amphitheatre formed by lofty surrounding mountains, and here is situate what is termed "The Sulphur." Though the scene was extremely grand and well worthy of observation, yet I confess I could not help feeling a good deal disappointed, as there was nothing like a crater to be seen, or any thing else that could lead me to suppose the place had any connexion with a volcano. On the north, east, and west sides were lofty mountains wooded to the tops, composed apparently of the same kind of porphyry we had noticed all along the way. On the south, the same kind of rock of no great height, quite bare of vegetation, and in a very peculiar state of decomposition. And on the south-eastern side, our path and the outlet into the ravine. The whole area thus included might be three or four hundred yards in length, and half that distance in breadth. The surface of the ground, not occupied by the ravine, was broken and strewed with fragments and masses of the porphyritic rock, for the most part so exceedingly decomposed as to be friable and to crumble on the smallest pressure. For some time I thought that this substance, which is perfectly white and in some instance exhibits an arrangement like crystals, was a peculiar mineral; but afterward became convinced, that it was merely the porphyritic rock singularly altered, not by the action of the air or weather, but, as I conjecture, by a strong sulphureous or sulphuric acid vapour, which is generated here, and which is probably driven more against one side by

West India firestone.

The Sulphur described.

No appearance of a volcano.

The rock decomposed by the sulphurous vapour.

by the eddy wind up the ravine, the breezes from any other quarter being shut out by the surrounding hills*.

This evolved from fissures

Admidst the loose stones and fragments of decomposed rock are many fissures and crevices, whence very strong sulphureous exhalations arise, and which are diffused to a considerable distance; these exhalations are so powerful as to impede respiration, and near any of the fissures are quite intolerable and suffocating. The buttons of my coat, and some silver and keys in my pockets were instantaneously discoloured. An intense degree of heat is at the same time evolved, which, added to the apprehension of the ground crumbling and giving way, renders it difficult and painful to walk near any of these fissures. The water of a rivulet, which flows down the sides of the mountain and passes over this place, is made to boil with violence, and becomes loaded with sulphureous impregnations. Other branches of the same rivulet, which do not pass immediately near these fissures, remain cool and limpid, and thus you may with one hand touch one rill which is at the boiling point, and with the other hand touch another rill which is of the usual temperature of water in that climate. The exhalations of sulphur do not at all times proceed fr m the same fissures, but new ones appear to be daily formed, others becoming

with intense heat.

Boiling rivulet.

Fissures continually varying.

* This peculiar decomposition of the surrounding rock has been frequently observed in similar situations, and under analogous circumstances, and has I find been accounted for by other persons in the same way: thus Dolomieu says, "The white colour of the stones in the interior of all the burning craters is owing to a real alteration of the lava produced by acid sulphureous vapours, which penetrate them, and combine with the alumine that constitutes their base, thus forming the alum obtained from volcanic substances." Voy. aux Isles de Lipari. p. 18.

And he afterward adds, "The alteration of lavas by acid sulphureous vapours is a kind of analysis of volcanic substances made by Nature herself. There are lavas, on which the vapours have not yet had sufficient time to act, so as to change their nature entirely; and then we see them in different states of decomposition, which we know by the colour."

Alum is doubtless formed at this place, as well as elsewhere under similar circumstances: the potash necessary for the composition of this salt being, as well as the argil, derived from the surrounding rock. See Vauquelin's Memoire. Journal des Mines, vol. x, p. 441.

ing, as it were, extinct. On the margins of these s, and indeed almost over the whole place, are to be nost beautiful crystallizations of sulphur, in many quite as fine and perfect as those from Vesuvius, or as any other specimens I have ever met with. The mass of decomposed rock in the vicinity is, in like er, quite penetrated by sulphur. The specimens I collected of the crystallized sulphur, as well as of composed and undecomposed porphyry, were left in-ently on board the packet at Falmouth, which prevents ving the pleasure of exhibiting them to the society. ot perceive at this place any trace of pyrites, or any metallic substance, except indeed two or three small ents of clay iron stone at a little distance, but did not er even this substance any where *in situ*. It is very ble that the bed of the glen or ravine might throw ight on the internal structure of the place, but it was ep, and its banks infinitely too precipitous for me to e down to it. I understood there was a similar ex-n and deposition of sulphur on the side of a mountain ore than a mile distant in a straight line; and a sub-ean communication is supposed to exist between the aces.

Sulphur beautifully crystallized.

No trace of pyrites.

Another Sulphur a mile distant.

ost every island in the western Archipelago, particu-ose which have the highest land, has in like manner its hur" or, as the French better express it, its "*Souffrière*." particularly the case with Nevis, St. Kit's, Guada- Dominica, Martinico, St. Lucia, and St. Vincent's. slands have several such places, analogous I presume to Montserrat; but in others, as Guadaloupe, St. Lucia, Vincent's, there are decided and well characterized vol-, which are occasionally active, and throw out ashes, , and lava, with flame. The volcano of St. Vincent's esented by Dr. Anderson, and others who have visited extremely large and magnificent, and would bear a rison with some of those of Europe. These circum-appear to have been entirely overlooked by geologists r speculations concerning the origin and formation of slands. It has indeed occurred to most persons, on surveying

Most islands in the western Archipelago have one or more,

and some have volcanoes.

General remarks on the island.

New species of ape.

Among the quadrumanous animals he has brought a black ape of a new species, with its young, and its skeleton; and the great slowpaced lemur [*le grand lori paresseux*], also with its skeleton.

Galeopithecus.

You know how rare the flying macauco, or pretended lemur volans of Linneus, is in collections. Neither Buffon nor Linneus ever saw it. Mr. L. has brought four of different ages, and two skeletons. The red and the variegated of some recent naturalists are only differences of age.

Bats, viverræ, felis.

He has five or six species of bats, two of which, at least, appear new to us; a new weasel; a new civet; and a new species of felis, in size approaching the lynx.

Skunk.

His most curious quadruped in our opinion is a new skunk [mouffette], truly belonging to that genus, hitherto supposed peculiar to America, like it striped with white on a black ground, but distinguished from the other species by being without a tail. It is common in the island of Java, and emits when pursued the same stinking smell as other skunks.

Squirrels and ichneumon.

He has also a new flying squirrel, a new ichneumon scarcely as big as a rat, and a new squirrel; beside many specimens of the Java squirrel, and of the taguan, or greatest flying squirrel.

Skeletons.

To these he has added the skeleton of a porcupine of Java, and those of two musks, which were wanting to your anatomical collection.

Birds.

Of birds Mr. Leschenault has brought over 130 species, which we have not been able to examine with sufficient minuteness to say how many are new.

Wild cocks.

There are however two different species of wild cocks, with their hens: one was discovered by Sonnerat, the other appears to us new. And we noticed a new bird of Paradise, black, with a very shining throat, among four other species.

Bird of Paradise.

Snakes

Of reptiles Mr. L. has brought a superb skeleton of a serpent, more than 15 feet [16f. Eng.] long, worthy a place in the finest collection. A specimen scarcely inferior to it, at least in rarity, is a well preserved skin of the celebrated achrocordus, or warted snake of Java. With these are about 30 other species of snakes, and several lizards; among which are the gecko of Java, and the blue guleot with its spindle-shaped eggs.

and lizards.

hard, a very well informed man, received him very courteously, and afforded him every assistance in his researches.

On the 24th of october Mr. L. set off from Samarang for Sourakorta, the residence of the emperor of Java, and about sixty miles south of the former place. On his journey he visited the mountains of Dounarang, Morbabou, Telomayo, and Marapi. The last has on its summit a volcano constantly emitting smoke. Tour in Java. Mountains. Volcano.

From Sourakorta he repaired to Djiokikorta, the residence of the sultan of Java. The sultan and the emperor are two independant princes. On this road, which is little more than forty miles, are some ruins of ancient temples, remarkable for their extent. Among these are a number of statues of lava, which seem to prove, that the people followed the religion of the bramins. Ancient temples. Statues.

A severe illness obliged him to return to Samarang. When he recovered, he visited the other parts of the island. After sailing to Madura, he returned to Java, and visited mount Idienne; a volcano, in which he found a lake, the water of which was strongly impregnated with sulphuric acid. He afterward sailed to the island of Bali. Volcano and sulphuric lake.

Having returned to Samarang, and packed up his collections, he repaired to Batavia in october 1806; sailed thence on the 27th of november on board an American vessel; arrived at Philadelphia in april 1807; sailed thence in june; and landed safe in France in july. Return home.

The following is an abstract of the account of his collection given to the museum of Natural History by Messrs. Cuvier, Desfontaines, and Lamarck. His collection.

We shall say nothing, observe these gentlemen, of the weapons, garments, and other articles used by the Indians, or of two very curious statues found in the ruins of a temple, as they do not pertain to natural history, and will find their place among the antiquities of the imperial library. Articles not belonging to natural history.

But Mr. L. has brought some articles interesting to the history of man: as some fragments of undoubtedly human bones brought from a burying place, that appear to have undergone at least a commencement of calcareous infiltration; and a scull of a Chinese of Java, that will increase our collection of those of different nations. Fossil human bones. Skull.

Among

[533 yds] across the widest part of its bottom. Here he perceived four openings, or mouths, near the top of the cavity, continually emitting clouds of sulphurous acid vapour, which, being condensed by the action of the cold air, fell into a great lake at the bottom, which is contained in the crater of the ancient volcano.

The waters in this basin, thus continually impregnated with the vapour, become so acid, that they attack every thing they touch; altering all the adjacent lava, and forming sulphate of iron and of lime, which they hold in solution, as well as sulphate of alumine. Accordingly when the rainy season arrives, the lake swells, overflows, and contaminates the water of the White river.

This may be obviated.

The cause being thus known, it is easy to obviate the noxious mixture of this water, by turning aside that which descends from the lake at certain seasons; and opposing obstacles sufficient to prevent its reaching the White river, which would thus remain constantly wholesome. This is a service of no small importance to the colony.

Analysis of the water.

Mr. Vauquelin has analysed the acid water of this lake, and found in it sulphuric acid, sulphurous acid, muriatic acid, sulphur, sulphate of potash, alum, and sulphate of iron.

XIII.

Analyses of Minerals: *by* MARTIN HENRY KLAPROTH, *Ph. D.* &c.

(Continued from p. 161).

Meadow iron ore.

WIESENERZ (meadow iron ore).

Black oxide of iron	66
Oxide of manganese	1·5
Phosphoric acid	8
Water	23
	98·5

Granular iron ore.

Pisiform ironstone from Hogau.

Oxide of iron	53
Silex	23
Alumine	6·5
Oxide of manganese	1
Water	14·5
	98

Granular

Granular chromated iron from Styria.

Chromate of iron.

Oxide of chrome	55·5
—— iron	33
Alumine	6
Silex	2
Loss in roasting	2
	98·5

Black manganese from Klapperud in Dalecarlia.

Manganese.

Oxide of maganese	60
Silex	25
Water	13
	98

Cerite from Bastnaes in Sweden.

Cerite.

Oxide of cerium	54·50
Silex	34·50
Oxide of iron	3·50
Lime	1·25
Water	5
	98·75

A fire-coloured opal, brought by Humboldt from Zimapan, in Peru.

Fire-coloured opal from Peru.

Silex	92
Water	7·75
Oxide of iron	0·25
	100

Brazilian topaz.

Brazilian topaz.

Silex	44·5
Alumine	47·5
Oxide of iron	0·5
Fluoric acid	7
	99·5

Saxon topaz.

Saxon topaz.

Silex	35
Alumine	59
Fluoric acid	5
Oxide of iron *a trace*	1
	100

Zoisite.

Crystallized zoisite.

Silex	45
Alumine	29
Lime	21
Oxide of iron	3
	98

Augite from Carniola.

Lamellar augite from Carniola.

Silex	52·50
Magnesia	12·50
Lime	9
Alumine	7·25
Oxide of iron	16·25
Potash	0·50
	98

and Sicily.

Scoriform augite from Sicily.

Silex	55
Alumine	16·50
Oxide of iron	13·75
Lime	10
Magnesia	1·75
Water	1·50
Manganese *a trace*	
	98·5

Apatite.

Conchoidal apatite, or spargelstein, from Zillerthal.

Lime	53·75
Phosphoric acid	46·25
	100

Columnar brownspar.

Stanglichen Braunspath (columnar brownspar), brought by Humboldt from Valenciana of Guanaxuato, in Mexico.

Carbonated lime	51·5
——— magnesia	32
——— iron	7·5
——— manganese	2
Water	5
	98

Dolomite

Dolomite from St. Gothard. Dolomite from St. Gothard,

Carbonated lime	52
——— magnesia	46·50
Oxided iron	0·50
——— manganese	0·25
Loss	0·75
	100

Dolomite of the Appennines:

A decomposed dolomite from Castelamare. from Castelamare, and

Carbonated lime	59
——— magnesia	40·5
Loss	0·5
	100

A dolomite in mass.

Carbonated lime	65
——— magnesia	35
	100

A dolomite from Carniola. from Carniola.

Carbonated lime	52
——— magnesia	48
Oxided iron	20·0*

Blue anhydrite, called muriacite, from Sulz on the Necker, spec. grav. 2·94. Anhydrous sulphate of lime from Sulz,

Lime	42
Sulphuric acid	57
Oxided iron	0·10
Silex	0·25
	99·35

Compact anhydrite, vulgarly tripestone, from Bochnia. Bochnia, and

Lime	42
Sulphuric acid	56·50
Muriate of Soda	0·25
	98·75

* Probably 0·20; although the other component parts, exclusive of this, amount to 100. C.

[redacted]

Hall in Tyrol.

Anhydrite from Hall, in Tyrol.

Lime	41·75
Sulphuric acid	55
Muriate of Soda	1
	97·75

agnesian spar.

Bitterspath (magnesian spar) from Hall in Tyrol.

Carbonated lime	68
——— magnesia	25·5
——— iron	1
Water	2
A mixture of clay	
	96·5

Terre verte from Verona,

Terre verte from Mount Baldo near Verona.

Silex	53
Oxided iron	28
Magnesia	2
Potash	10
Water	6
	99

Cyprus,

Terre verte from Cyprus.

Silex	51·5
Oxided iron	20·5
Magnesia	1·5
Potash	18
Water	8
	99·5

and West Prussia.

Terre verte from New West Prussia.

Silex	51
Alumine	12
Lime	3·5
Magnesia	2·5
Oxided iron	17
Soda with a suspicion of potash	4·5
Water	9
	99·5

Alumstone

Alumstone from la Tolfa. Alumstone from la Tolfa.

Silex	59·5
Alumine	19
Sulphuric acid	16·5
Potash	4
Water	3
	102

Alumstone from Hungary. Hungary,

Silex	62·25
Alumine	17·50
Sulphuric acid	12·50
Potash	1
Water	5
	98·25

Aluminous earthy schist from Freyenwalde. and Freyenwalde.

Sulphur	28·5
Charcoal	196·5
Alumine	160
Silex	400
Black oxide of iron, with a trace of manganese	64
Sulphate of iron	18
Gypsum	15
Magnesia	2
Sulphate of potash	15
Muriate of potash	5
Water	107·5
	1011·5

Jade from Switzerland, hemanite of Delamétherie, saussurite of Saussure. Jade.

Silex	49
Alumine	24
Lime	10·50
Magnesia	3·75
Oxide of iron	6·50
Soda	5·50
	99·25

Lazulite

[redacted]

Lazulite. Lazulite from Krieglach in Styria.

Alumine	71
Silex	14
Magnesia	5
Lime	3
Oxide of iron	0·75
Potash	0·25
Water	5
	99

Moya. Moya from Quito, brought over by Humboldt, 100 grs. yielded

		Cubic inches.
Carbonic acid gas	2·25	= 1·06
Hidrogen gas	14·50	= 0·36
Water and ammonia with some empyreumatic oil		11
Charcoal		5·25
Silex		46·50
Alumine		11·50
Lime		6·50
Oxide of iron		6·21
Soda		2·50
		90·88

Guano. Guano from the islands on the coast of Peru, brought over by Humboldt. This guano is supposed to be the remains of the excrements of the birds, with which those islands are covered.

Ammoniacal uric acid	16
Phosphated lime	10
Oxalated lime	12·75
Silex	4
Muriated soda	0·50
Mixed sand	28
Water with an animal residuum, and loss	26·75
	98

Klebschiefer,

Klebschiefer, or polishing slate, from Menilmontant. Polishing slate.

Silex	62·50
Lime	8
Oxide of iron	4
Charcoal	0·75
Alumine	0·50
Lime	0·25
Water and gasses evolved	22
	98

Olive-green garnets from Siberia. Olive green garnets.

Silex	44
Lime	33·5
Alumine	8·5
Oxide of iron	12
——— manganese, *a trace*	
Loss	2
	100

Green chalcedony from Olympus, near Prusa, in Asia minor. Green chalcedony.

Silex	96·75
Oxide of iron	0·50
Alumine	0·25
Water	2·50
	100

True Lemnian earth. Lemnian earth.

Silex	66
Alumine	14·50
Oxide of iron	6
Lime	0·25
Magnesia	0·25
Soda	3·50
Water	8·50
	99

Fuller's

Fuller's earth, English.

Fuller's earth from England.

Silex	53
Alumine	10
Oxide of iron	9·75
Magnesia	1·25
Lime	0·50
Sea salt	0·10
Water	24
Potash, *a trace*	
	98·6

and Siberian.

Red fuller's earth from Siberia.

Silex	48·5
Alumine	15·5
Magnesia	1·5
Oxide of iron	6·5
——— manganese	0·5
Water	25·5
Sea-salt, *a trace*	
	98

Red earth of Sinope.

Earth of Sinope, in Pontus, brought from Natolia by Mr. Hawkins. This earth, according to Pliny, formed red paint.

Silex	32
Alumine	26·5
Oxide of iron	21
Sea salt	1·5
Water	17
	98

Tincal.

Tincal, crystallized borax.

Boracic acid	37
Soda	14·5
Water of crystallization	47
	98·5

Datholite

Datholite. Datholite.

Silex	36·5	
Lime	35·5	
Boracic acid	24	
Water	4	
Iron and manganese, *a trace*		
	100	

Fluor. Fluor.

Lime	67·75	
Fluoric acid	32·15	
Oxide of iron, *a trace*		
	99·9	

(To be continued.)

SCIENTIFIC NEWS.

Geological Society.

JUNE the 5th. An account of some new varieties of Alcyonia found in the Isle of Wight by Thomas Webster, Esq. Member of the Geological Society, was read. In viewing the rocks about Ventnor Cove, and in various parts of the undercliff, Mr. Webster remarked, in the sandstone stratum immediately under the chalk marl, a great number of small prominences, resembling in form the branches of trees. They were of various sizes, from half an inch to three or four inches in diameter; their substance was sandstone, of the same kind as the rock they were in; but the part resembling the bark was somewhat harder, which enabled it to endure longer than the rest of the stone, and thus project above its surface. Some of them were straight, others a little crooked, and in a few instances he observed them forked. He found fragments of these bodies in every part of the island, where the sandstone stratum can be seen, and particularly among the masses of rock lying under the cliffs of Western Lines. In this last place he found, that the stems above described had frequently heads or bulbous terminations attached to them, in form somewhat resembling a closed tulip; and in some of these he found distinct traces of organic

New varieties of alcyonia.

ganic structure; from which it appeared, that these heads consisted of a group of *tubuli*, now converted into, and enveloped with stony matter. Beside these extraordinary shapes, which projected in relief, Mr. W. observed a variety of very regular white figures, as if painted upon the rock, being even with the surface. They consisted of circles from two inches to half an inch in diameter, ellipses of various eccentricities, and parallel lines both straight and curved. By a careful examination Mr. W. found, that these white figures belonged to the other class of bodies already described; and that the cylinders were only the internal parts of the same body, the sections of which formed the white circular and elliptical figures. The vast masses of rock, which have fallen down, having separated from the cliff at the divisions between the beds, showed their upper and under surfaces covered with layers of these bodies heaped upon each other, and lying prostrate in every possible direction: and in the joints between the beds, where they were still not separated, they were distinctly seen. The green sandstone and the limestone he found to be the chief repositories of these bodies; in the ferruginous sand below the green sandstone he found none, and only a few fragments of cylinders in the blue marl on which the sandstone rests. He traced them upwards into the chert, but they there became rare, and they totally disappeared in the chalk marl. He found them however frequently in the fragments of flint lying on the shore. Mr. Webster having brought away an extensive series of specimens, which he has since deposited in the collection of the Society, submitted them to the examination of Mr. Parkinson, who is of opinion, that they belong to the genus *alcyonium*, but that they are of three or four different species, neither of which has been hitherto described. From the resemblance which these bodies bear to a closed tulip attached to its stalk, Mr. Webster suggests, that the name of *tulip alcyonium* may not be improperly applied.

Hippurites from Sicily. Some observations by James Parkinson, Esq. Mem. G. S. on the specimens of *Hippurites* from Sicily, presented to the Society by the Hon. Henry Grey Bennet, Mem. G. S. were read. These specimens Mr. P. considers to be such as demand particular attention, as they possess those characters,

racters, which will probably serve to correct some erroneous opinions respecting the nature and habits of the animals of which these shells were the dwellings. One of the specimens contains a nearly perfect shell, longitudinally divided so as to display the two ridges, with the numerous septa and chambers. From an examination of the specimens, and by comparing them with the observations he has before had an opportunity of making, Mr. Parkinson is of opinion, that the structure of the shell of the hippurites is such, as would enable the animal to raise itself to the surface of the water. This opinion is in opposition to that of Mr. Denys de Montfort, and most of the French oryctologists, who consider the hippurites as belonging to what they term *pelagian shells*, or such as constantly inhabit the bottom of the sea, never rising to the surface, or appearing on the shore; and therefore, that there is no reason to suppose them belonging to animals which are now extinct; but only, that their recent analogues have not yet been brought to view.

June the 19th. A paper by Joseph Skey, M. D., entitled "Some remarks upon the Structure of Barbadoes as connected with specimens of its Rocks," communicated by Arthur Aikin, Esq., sec. was read; together with a note by Mr. Parkinson on some of the specimens presented by Dr. Skey. The island of Barbadoes is totally unlike those immediately near it, both in structure and in appearance, the land rises in a gentle swell from the coast towards the middle of the island, except in one small district: its highest hills do not exceed 800 or 900 feet, and their general direction is nearly N. W. and S. E. Upon the N. Eastern coast the shores are bolder than in the other parts of the island, as is the case in many of the islands of those seas. Barbadoes is composed of limestone, in great part of fossil madrepores, and traces of organic structure are to be met with in almost every part of the island, more particularly along the whole of the S. and S. W. coast. The land, which when seen from the sea, appears to rise uniformly from the coast, is observed on a nearer view, to consist of successive terraces rising in two or three gradations, one above the other, each forming a plain of a quarter or half a mile in breadth, and terminated by

Island of Barbadoes.

quartz and porphyry. On the edge of these rocks are found large detached masses of puddingstone, consisting of rounded pebbles of greenstone of different varieties, of amygdaloid and quartz cemented by a paste, which appears to consist chiefly of trapsand united by the hard variety of calcareous spar. The paste contains also, in small quantity, zeolite, prehnite, garnet, and diallage. This puddingstone, where nearest to the fort, is at least half a mile distant from it. The walls of the fort consist principally of granite, gneiss, mica-slate, clay-slate, quartz, puddingstone, and pyritical slate, entangled together; with a very small proportion of the particular rock on which the fort itself is founded: the puddingstone forming the greater part of them. This puddingstone Dr. M. shows to be the only vitrifiable ingredient of the walls; and from the distance from which it must have been brought, and the great quantity of it employed in the work, he considers it probable, that the builders of the fort must have been acquainted with its vitrifiable nature, and that it was on account of this quality, that they had employed so great labour in transporting it. For if their object had not been to produce vitrification, but merely to erect a dry wall of stone, the limestone of the hill would have answered their intentions, or perhaps the loose stones of the adjoining plain. That they did not obtain the puddingstone from the latter source is evident; for although the plain and shore are covered with fragments, these consist almost entirely of the primary rocks; and besides, the pieces of the wall which have not felt the fire are angular fragments, showing pretty clearly, that they were not collected on an alluvial plain, but broken from the rocks where they are found. Dr. M. next proceeds to describe the various states in which the different stones are found. The puddingstone exhibits the greatest variety of changes, it is found in every state, from a black glass to a spongy scoria capable of floating in water, sometimes exhibiting the gradual succession of changes from incipient calcination to complete fusion. To ascertain the degree of heat necessary to produce the corresponding changes in this rock, Dr. M. submitted various parts of it to the furnace, and he found, that some of the fused substances must have been brought

The vitrification probably the effect of design.

to

sand of which the outside of the tube consists. Both the sand and the vitreous part of the tube scratch glass; and on the latter, when viewed by a lens, there are seen small air blebs, such as are common to imperfect vitrification. Both are insoluble in sulphuric and nitric acids; infusible before the blowpipe without addition; partially fusible on the addition of boracic acid but; with soda a complete fusion took place, and the residue was nearly soluble in water.

Vitrified forts in Scotland.

A paper by Dr. Mac Culloch, M. G. S. on the vitrified fort of Dun Mac Sniochain, near Oban in Argyleshire, was read. In the discussion which some time ago took place respecting the vitrified forts of Scotland, the question on which the two contending parties were most at issue was, whether the vitrification was the effect of design or of accident. It occurred to Dr. M., that light might be thrown on the subject by examining with mineralogical accuracy the substances of which these structures were composed; and noting the changes, which each had undergone, in consequence of the fire; and also by observing whence the stones had been derived, which were used in them. And that the question of accident or design might be illustrated, by examining in the laboratory the degree of heat required to produce the appearances in the stones, which actually exist in these structures.

Fort of Dun Mac Sniochain

The fort of Dun Mac Sniochain stands on a long narrow hill, which is nearly precipitous along three parts of its circumference; and at the other end it rises from the plain with a very accessible acclivity. The walls, which are nearly all at present buried under the soil, are about eight or ten feet in thickness. They bear marks of vitrification through their whole extent, but in no case does it appear to have extended more than a foot or two upwards, and the most perfect slags are found at the bottom of the foundation. In the higher parts there are stones roasted by the action of the heat, but unvitrified; and at length the marks of fire almost entirely disappear. The hill consists of alternate beds of schistus and limestone, but the latter is the predominant rock. It is perfectly insulated in a great alluvial plain. The mountains of Benediraloch, which bound the plain to the west, consist of granite, gneiss, mica-slate,

quartz

Objects for which the society intend to present premiums and medals.

New varieties of the gooseberry, which might supply the markets with green fruit at earlier periods, and mature fruit at earlier and later periods than those now cultivated.

New varieties of pears, similar to those which have been introduced from France; but sufficiently hardy to grow and ripen on standard trees, and calculated to supply the markets at a moderate price during winter and spring.

A good and early new variety of grape, better adapted to the climate of Great Britain, in the open air, than any now known.

Better and more productive varieties of the apple, and capable of being longer preserved in perfection, than most hitherto known.

A good early nectarine; a variety of the strawberry earlier than the common scarlet; and of the cherry, which would ripen before the early may.

More early and hardier varieties of the peach, which might succeed better, at least, than any now known, on standard or espalier trees.

Several native varieties of the plum afford blossoms so hardy, that they are rarely injured by frost. Might not rich varieties be obtained by introducing the farina of the fine but tender kinds into the prepared blossoms of these? It is stated, in the Pomona Herefordiensis, that very rich and very hardy varieties of the apple have been thus obtained immediately from the seeds of the Siberian crab.

In pointing out the preceding objects, as deserving the attention of gardeners, it is not the intention of the society to limit its patronage to those solely: on the contrary, it is their wish, to promote and encourage successful experiments, in every branch of useful and ornamental horticulture.

To Correspondents.

The communications from Mrs. Ibbetson and E. G. shall be inserted the earliest opportunity.

A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

SUPPLEMENT TO VOL. XXXII.

ARTICLE I.

***Observations on the Disease in the Potato, generally called the Curl; pointing out the most probable Method of preventing it; with an Account of the Results of a few Experiments made on the Subject. By Mr.* THOMAS DICKSON, *Leith Walk, Edinburgh* *.**

THIS disease, so far as I can learn, first began to be alarming to the growers of the potato about thirty-five or forty-years ago. Since that time, it has continued to engage the attention of many eminent agriculturists and gardeners. Date of the curl in potatoes.

Various opinions have at different times been advanced as to its cause. Some were of opinion, that the disease was caused by the tubers used for seed-stock not having been sufficiently ripened:—others thought, that they had been frost-bitten, in the course of the preceding winter:—some ascribed the evil to the effects of blights attacking the plants in coming through the ground;—others to the attacks of certain minute insects:—lastly, the exhausted state of the soil was blamed for the disease. But no one seems to have hit upon the real cause, until the honourable Baron Hepburn of Smeaton, in East Lothian, one of Opinions respecting its cause. Real cause.

* Memoirs of the Caledonian Horticultural Society, vol. I, p. 49.

the most successful and intelligent agriculturists of this country, started a new theory on the subject; which, from its singularity, and *seeming* inconsistence with our experience in matters of a similar nature, did not at the time meet with that attention, to which it undoubtedly was entitled. The Baron thought, that the curl was probably caused by the tubers used for seed-stock having been allowed to become too ripe the preceding year; and that this practice of overripening, being repeated year after year, was the real cause of the disease, the vegetative power in the tubers being thus exhausted.

Experiments made with a view to its proof.

I candidly confess myself to have been rather at first a sceptic on the subject; but, after considering the thing a little, my doubts began to clear away. In order to satisfy myself thoroughly, I resolved upon making a suite of experiments. I accordingly did so; and as they were conducted entirely by myself, or under my own immediate superintendence, I can pledge myself for their accuracy. I now beg leave to lay them before the Caledonian Horticultural Society, in hopes that they may, by means of the Society, be made known to the public; and as the experiments are easily repeated, that they may induce others to turn their attention to the subject.

I think it right to observe, that the experiments now to be detailed were not made with any view of their ever appearing before the public; nor would they have been brought forward at this time, but from a wish to promote the views of this Society.

Propagation of particular varieties by cuts.

It is well known to all cultivators of the potato, that the usual mode of reproducing any particular variety of this valuable root is by cuts or sets of the tubers; and that this mode of propagation is repeated every year, so long as that particular sort is wished for, without our ever thinking of reinvigorating the *seed-stock* *, by raising new plants from the real seeds. In this way it happens, that merely the individual *variety* is propagated; the *species* being reproduced only by sowing the true seeds of the plant. It

* By this expression is always to be understood the stock of *tubers* for planting, in contradistinction to the real seed of the plant.

is only by sowing the seeds that we obtain *new* varieties. But if the seeds be taken from any particular variety, that is wished to be preserved, and if care be exercised, that the plants shall have no communication with the farina of any other plants of the same species in flower, then the produce of these seeds will probably be the same, or nearly the same, with that variety from which the seeds were saved; and from the seed-stock being renewed and reinvigorated in this way, it seems likely, that the variety so obtained may, by observing a proper management, be preserved from the curl or any other kind of degeneracy, for any length of time.

May be effected by seeds with care.

I shall presume, that the principal cause of the curl in the potato is the overripening of the seed-stock for the supply of the ensuing year, by allowing it to remain too long in the ground, and especially if it be also planted early; this practice, being repeated for several years successively, causes an exhaustion of the vegetative principle in the tubers, which renders them totally unfit to produce vigorous healthy plants; and is the principal cause of the disease. This doctrine has almost uniformly been objected to by many very intelligent agriculturists and gardeners, as being quite contrary to our experience in regard to seeds in general; full ripeness being considered the best recommendation. But this objection, I apprehend, arises from the taking an improper view of the subject. It is true, that all of what are properly called *seeds* are improved, by being thoroughly ripened; but *cuts* or *sets*, taken from the tubers of a potato, cannot, strictly speaking, come under the description of seeds. Planting cuts of the potato is analogous to budding or grafting of trees, being only a secondary mode of propagation; and, consequently, the above-mentioned objection does not hold good. This doctrine may be farther illustrated, by observing the strong tendency, which potatoes raised from seeds have to run to flower and seed, unless prevented, by destroying these as they appear, and by earthing up the roots of the plant, so as to induce them to throw out tubers. This natural disposition of plants raised from seeds will remain for several generations of the plant, gradually yielding to the artificial

Curl produced by using overripe potatoes for sets.

Objection to this hypothesis answered.

Growth of potatoes raised from seed.

 means

Argument from successful practice; means used, until they at last become what we wish. And what may be deemed still a farther proof is, that those who cultivate potatoes most successfully, in the low and early parts of this country, where the disease chiefly exists, bring a supply of seed-stock from the higher and later parts of the country, for a change, every second year at farthest. In such high places, from the lateness and wetness of the climate, the farmers are prevented from planting their potatoes so early as in the low country, and are also, from the fear of early frosts, obliged to take up their crop sooner; consequently the tubers are never so highly ripened as to weaken the vegetative principle in them. Here, then, we have a strong practical testimony to the truth of the docrine which has been advanced.

and the effects of its opposite. On the other hand, in the early districts of the low country, where, as has already been remarked, the disease is principally known, particular kinds of potatoes are planted year after year successively, from the same seed-stock; and most of the early kinds are planted soon in the season, with a view to procure an early crop for the market: a part of these is generally allowed to remain in the ground till the usual time of taking up, to supply seed-stock for the ensuing year: by this time, however, the plants have become so ripe as to weaken very much the vegetative power of the tubers. This practice being repeated for several years, at last so impairs the vegetative power in the tubers, as to produce the curl; and there is no doubt, that, if this practice were persevered in, it would ultimately destroy the power of vegetation altogether, as I have proved by experiments.

Potatoes to be used for setting should never be suffered to flower. There is yet another powerful cause, which weakens the vegetative power in the tubers; and this is, the allowing such plants as are intended to supply seed-stock for the ensuing year, to run to flower, and produce seed*. This should in all cases be prevented, by cutting off the flowers as they appear, even in embryo. Thus, by turning nature from her ordinary course, we force her to exert herself in

* It is generally the late sorts of potatoes, that produce seeds, very few of the early kinds doing so.

another

another channel, and to throw back into the tubers that portion of the vital principle of the plant, which would have been exhausted in the formation of flowers and seeds. Nothing will more contribute to prevent degeneracy in the potato, and especially to prevent curl, than this treatment.

Experiments by the author.

In proof of what I have already advanced on this subject, I shall now state a few experiments made by myself in the years 1801,-2,-3. They appear to me to be quite conclusive, and will go farther to convince, than a volume written without experiments.

In the autumn of 1800, when in Fife, at a friend's house, I met with a potato of the long flat kind *, which I thought very excellent, and obtained a few to cultivate for my own use: he however informed me, that they had been so infested with the curl for some years, that he had resolved to abandon the culture of them altogether. This led me to conclude, that, from their shape, &c., they were well adapted for being made the subject of some experiments I had previously resolved to make, with a view to ascertain the truth of the new idea, upon the cause of the curl, which had been some time before mentioned to me. Accordingly, I selected about half a peck (14lb.) of these, as near the size and shape of the annexed sketch as possible. I took one or two sets from each end of each potato, that is, from the extreme, or dry end, and from the umbilical, or wet end, next the connecting radicle: each sort was planted upon the same ground, but in different rows, with the same kind and quantity of manure to each, and in every respect in exactly the same circumstances, on the 27th of April, 1801.

Sets taken from the opposite ends of potatoes.

Results.

The season was very favourable. Upon examining the plants about the end of June, I found, that all those that were taken from the wet, or least ripened end of the potato, had come up, and were looking well and healthy, except three plants, which were a little affected with the disease: these I threw out, preserving only such as were quite free from it. Upon examining those plants, which were

* A sketch of a tuber of this kind, of the natural size, accompanies this, showing the different cuts or sets, &c.

produced

produced from the dry or ripest end of the potato, I found, that but few of them had appeared above ground, and such as had were all diseased, more or less; but in many instances, the sets had not vegetated at all, nor did they, upon taking them up to examine them, show any signs of vegetation; although quite sound and fresh, they were quite inert; nor did these change their appearance throughout the season, being nearly as fresh when the rest of the crop was lifted, as when they were put into the ground.

On the 30th of July, the whole were again examined; the plants from the unripe sets were almost covering the ground, though planted at two feet between the rows, and were looking well, remarkably free from curl, and promising an abundant crop; while those from the ripened sets, which had vegetated, and had grown, had made very little progress indeed, and were universally curled; several of the plants died after coming a certain length, seemingly from mere weakness; and such as grew stronger had very few tubers at their roots, and these very small and puny.

On the 3rd of October, I took up the produce of both sorts, and pitted them, for renewing the experiment the ensuing year.

The experiment twice repeated.

The same course of experiment was accordingly repeated, not only next year (1802), but also the following year (1803); and the results were exactly similar; the plants produced from the wet, or unripened ends, continuing healthy, and producing abundant crops, while those produced from the dry ends continued to degenerate.

I thus satisfied myself, that the disease originated entirely in the overripening of the seed-stock; and indeed all my experience, since these trials were made, has tended only to strengthen this opinion. I might follow out this to a much greater length, and supply many more facts, all calculated to prove the truth of what has already been advanced; but, by doing so, I should only multiply the detail of similar trials and facts, which, instead of inducing individuals, might rather deter them from satisfying themselves by making experiments. This I should wish them to do.

It

It may be proper to observe, that the produce of the curled potatoes was taken up before being too ripe, and replanted with the others: I cannot say that the disease was removed, but they did not get worse. Perhaps replanting them in very highly manured land, for several years, might have a good effect: but unless it were for the sake of reclaiming a favourite variety, the experiment is hardly worth making.

Perhaps the curl might be removed by taking up the tubers early.

Having trespassed so long on the attention of the Society, I shall only beg leave to suggest a few simple rules, which, if attended to, will, I am humbly confident, soon entirely banish the disease of curl from the country. These are,

Means of preventing the curl.

1. To procure a sound healthy seed-stock, which cannot be relied on, unless obtained from a part of the high country, where, from the climate and other circumstances, the tubers are never overripened.

2. To plant such potatoes as are intended to supply seed-stock for the ensuing season at least a fortnight later than those planted for crop, and to take them up whenever the *haulm* or stems become of a yellow-green colour: at this period, the cuticle or outer skin of the tubers may be easily rubbed off between the finger and thumb.

3. To prevent those plants, that are intended to produce seed-stock for the ensuing year, from producing flowers or seeds, by cutting them off in embryo; taking care, however, to take no more off than the extreme tops, as, by taking more, the crop may be injured. The best mode of doing this, is with a common reaping-hook, or light switching bill. Two boys or girls may do an English acre in two or three days.

Nurseries, Leith Walk,
6th of March, 1810.

References to Plate VIII, *Fig.* 1.

A The ripened or dry end.
B The waxy or wet end.
a a The cuts or sets from the dry end.
b b The cuts or sets from the umbilical end.
c c The umbilical cord or connecting radicle.
d d The real roots of the plant.

II. *Electric*

II.

Electric Attractions and Repulsions are not explained in a Satisfactory Manner in the Hypothesis of Two Fluids. By J. C. Delametherie *.

Electrical attractions and repulsions said to be easily explained on the supposition of two fluids.

"ELECTRICAL attractions and repulsions," says the author † of the Elementary Treatise on Physics, vol. I, p. 590, 2d ed., "form one of those subjects, that have most engaged the attention of philosophers, and have most embarrassed those, who have endeavoured to refer to the action of a single fluid two diametrically opposite effects, which frequently succeed each other very rapidly in the same body. But if we admit the combined action of two fluids, the theory acquires such a happy simplicity, that the simple enunciation of the hypothesis seems to be a concise explanation of the phenomena.

"Mutual repulsion of two bodies, the electricities of which are homogeneal.

Instance in repulsion.

"§ 557. If we suppose in the first place two bodies, each electrified by an additional portion of vitreous or resinous electricity, that has been transmitted to it, we see instantly what must take place; since this principle, that bodies animated with the same kind of electricity repel each other, and that bodies solicited by different electricities attract each other, is only as it were a literal translation of that other fundamental principle, *that the particles of each of the component fluids act on one another by repellent forces, and exert attractive forces on the particles of the other fluid.*

This explained experimentally.

"§ 558. This however requires some details, which will find their place in the exposition we are about to give of the means, that may be employed to prove this principle by experiment. Let A B, *Pl.* VIII, *Fig.* 2, be two balls of pith of elder, or any other conducting matter, suspended by threads at a small distance from each other, and to which the vitreous electricity has been communicated. The fluids surrounding these balls mutually repel each other;

* Journ. de Phys. vol. LXV, p. 315.

† Mr. Haüy.

and

and their particles would be diffused through space by opposite movements, if the surrounding air did not retain them near each body. Hence they can only glide on the surface of the body, so, for instance, that the fluid of the body A, being crowded toward the posterior part of this body, *d*, will exert its effort on the air itself, that is adjacent to this part. Thus the equilibrium between this air and that contiguous to the anterior part, *c*, being broken, *the latter will act by its elasticity on the body* A, *to impel it in the direction c h*. The same reasoning applies in the opposite direction to the body B; whence we conclude, that the fluids and the bodies, or balls, impelled by a common movement, must recede from each other. We should have a similar result, supposing the two bodies to be electrified resinously.

"Mutual attraction of two bodies, the electricities of which are heterogeneous.

"§ 559. Let us imagine, that, one of the two bodies, A for instance, being solicited by the vitreous electricity, the electricity of the other, B, is resinous. The fluids then will attract each other; so that, with respect to the body A, which we shall continue to take as the object of comparison, the crowding will take place toward the anterior part of the body, *c*. The fluid accumulated in this place then will act repellently on the neighbouring air: whence it follows, *that the air contiguous to the posterior part, d, will impel the body in the direction d n*. The same effect will take place in an opposite direction with respect to the body B, and thus the fluids and the bodies will be carried toward each other." Instance in attraction.

What has been said is copied literally, that the author's opinion might not be misrepresented.

This explanation of electric attractions and repulsions by the action of two fluids does not appear to me satisfactory. I conceive it may be refuted by a single experiment. The two experiments related by the author, § § 558 and 559, succeed as well in the vacuum of an airpump, as in the open air: consequently neither the attraction nor the repulsion of the two little balls is produced by the action of the atmospheric air. Proof that this explanation is erroneous:

In

and that the contrary should take place if the supposition were true.

In the next place I would observe, that, if we suppose the air to be driven back by the electric fluid gliding over the ball, the effect should be the reverse of what takes place: for, as this electric fluid acts with sufficient force against the air to drive it back, and the little ball is very moveable, the same thing should take place as with an eolipile, sky-rocket, &c. The sky-rocket, for example, ascends in the air only because the powder as it burns makes a continual jet, which strikes and drives back the air with great velocity. The air resists this rapid movement; and the rocket, being movable is driven forward, and proceeds with more or less rapidity. This is also the cause of the recoil of cannons, muskets, &c.

III.

Memoir on the Proportion the Evaporation of Water bears to the Humidity of the Air. By HONORE FLAUGERGUES*.

Humidity of the air influences evaporation.

BY the humidity of the air should be understood the proportion, that the quantity of water mingled and suspended in a given quantity of air bears to the quantity of the air. The more humid the air, the slower and less considerable the evaporation: and there is even a degree of humidity, at which evaporation wholly ceases, because, the air being loaded with all the moisture it can contain, no more can rise. I have made a great number of experiments, to ascertain the law, that this decrease of evaporation follows; and as experiments of this kind appeared to me most proper to determine the general law of evaporation as it respects the moisture of the air, I shall confine myself here to those, on the results of which most dependance can be placed, as I employed in making them an extensive apparatus, of which the following is a description.

Experiments to ascertain the law of its action.

Apparatus described.

I began with procuring a stock of air completely dried for the purpose in a very dry season. I caused a cask to be hooped and headed, that would contain about nine cubic

* Journ. de Physique, vol. LXX, p. 157.

feet,

feet, very clean, and the staves and heads of which were perfectly dry. I luted the joints accurately; and for greater precaution pasted over them slips of paper, to prevent all access of the external air. Into the bung-hole of this cask I poured about two cubic feet of quicklime coarsely powdered, and hot from the kiln. The bung-hole was then closed with a very tight straight cock, the key of which was perforated with a hole about three lines in diameter. In this state I left things about three weeks, shaking the cask several times a day, that the lime might present fresh surfaces to the contact of the air, and thus more speedily and completely free it from all the water, that might be suspended in it. Thus presuming the air to be perfectly dried, I employed it in my experiments, pouring it into a tin vessel, which I had made for the purpose. Preparation of the air.

This vessel consists of a hollow cylinder, 13 in. 2 lines* in diameter, and 18 inches high. This cylinder is closed at one end by a circular plane, and at the other by a cone 3 in. 7 lines high. Its capacity is about 2614 cub. in. The summit of the cone is truncated; and has soldered to it a small cylindrical tube, capable of receiving a cylindrical vessel of glass, four lines in diameter, intended to hold the water to be evaporated into the air in the vessel. This glass is fixed in the tube by means either of putty or of soft wax. The tin vessel may be fixed in a perpendicular position, with the glass vessel downwards, by means of two handles soldered to the cylindrical part, and resting on two supports of iron fixed upright on a table. The sheets of tin, of which this instrument is made, are very carefully soldered; and the external air cannot find admittance, except through the aperture of the tube at the summit of the cone when this is not closed by the glass vessel. Vessel for making the experiments.

To fill this vessel with the dry air in the cask, I began by filling it completely with fine sand, perfectly dried by a fire. I then placed it on the cask, so that the aperture at the end of the conical part was exactly fitted to the cock; and, after carefully closing the juncture with soft wax, I turned the key of the cock. The sand, escaping by the hole in Mode of filling the vessel.

* The French measures are here retained. C.

the key, ran into the cask, and the air displaced by it at the same time ascended into the vessel. When the sand, by the help of a few slight shakes, had entirely run out, and the vessel was filled with the dry air of the cask, I shut the cock, carefully removed the tin vessel, and immediately introduced into the tube the orifice of the cylindrical glass, filled to within three lines of the top with very pure rain water; and cemented it there so that it could not fall, and that none of the external air could get in. I then placed this vessel, keeping it always upright in the way I have mentioned, in a room, where I kept up a uniform temperature during each series of experiments. The glass cylinder at the bottom of the vessel being thus completely isolated before the window of the room, it was easy to measure the sinking of the water in it by evaporation, by taking with a pair of very pointed spring compasses, and with the assistance of a good lens, the distance from the surface of the water to the level at which it stood at the commencement of the experiment, this being marked on the glass with a diamond. For calculating this distance I employed the same scale of a thousand parts, made by Canivet, as I used for my experiments on the relation between heat and spontaneous evaporation *. Every day at the same hour, four o'clock mean time, I took the measure of the fall of the water below its original level: but in the following table, to save room, I shall set down only the measures of every third day, confining myself also to the four series of experiments that succeeded best.

Manner of making the experiments.

During the first of these series, the thermometer by the side of the tin vessel was constantly at 20° [77° F.]; and the height of the barometer, when the vessel was filled with the air, was 27 in. 9·7 lines [29·64 in. Eng.].

Heat and pressure during the experiments.

In the 2nd series the thermometer continued with very little variation at 18° [72·5° F.]; and the height of the barometer at the commencement was 28 in. 0·4 l. [29·88 in.]

During the 3rd series the thermometer marked nearly 15° [65·75° F.]; and the height of the barometer 27 in. 8·4 l. [29·52 in.].

* J. de Phys. vol. LXV, p. 446: or Journal, vol. XXVII, p. 17. On this scale 190 parts were equal to a Paris inch.

In

In the 4th series the thermometer was constantly at 10° ;4·5° F.]; and the barometer at the beginning was at 3 in. 2·9 l. [30·1 in.].

The results of these experiments are given in the follow- g table, divided into four columns. The first of these ontains the date; the second, the distance below the level the commencement; the third, the distance fallen be- veen the times of measuring. These differences form ap- arently a decreasing geometrical progression, as I quickly erceived: but to render this law more evident I have added fourth column, in which the differences are calculated by serting five mean geometrical proportionals between the rst and last difference found by experiment.

The table explained.

Tabulated Results.

)ate of the measures. 1807.	Fall of the water.	Actu- al dif- fer- ences.	Differ- ences calcu- lated.	Date of the measures. 1807.	Fall of the water.	Actu- al dif- fer- ences.	Differ- ences calcu- lated.
First Series.				Third Series.			
Aug. 1	Parts 0			Sept. 18	Parts 0		
4	59	59	59	21	35	35	35
7	96	37	35·9	24	55	20	21·7
10	117	21	21·9	27	70	15	13·5
13	132	15	13·3	30	78	8	8·4
16	140	8	8·1	Oct. 3	82	4	5·2
19	145	5	4·9	6	86	4	3·2
22	148	3	3	9	88	2	2
Second Series.				Fourth Series.			
Aug. 24	Parts 0			Oct. 12	Parts 0		
27	47	47	47	15	24	24	24
30	73	26	27·8	18	37	13	14·1
Sept. 2	91	18	16·4	21	44	7	8·3
5	102	11	9·7	24	50	6	4·9
8	108	6	5·7	27	53	3	2·9
11	111	3	3·4	30	54	1	1·7
14	113	2	2	Nov. 2	55	1	1

As

The evaporation decreased in geometrical progression.

As the vessel containing the water evaporated in the [pre]ceding experiments was cylindrical, the successive dim[inu]tions of the height of the water are proportional to [the] quantities evaporated; and as these successive diminu[tions] form a decreasing geometrical progression, as may e[asily] be verified, we shall conclude, that the quantities of w[ater] evaporated in equal times, in the same body of air, l[ike]wise form a decreasing geometrical progression. Hen[ce it] is easy to ascertain the law followed in the evaporatio[n of] water with respect to the humidity of the air, by mea[ns of] a very simple geometrical construction.

The law exemplified by a diagram.

Let H I, *Pl.* VIII, *Fig.* 3, be an hyperbola, descr[ibed] between the rectangular assymptotes C A, C K. On [the] assymptote C A take the abscissas C A, C B, C D, C [E], &c., in a decreasing geometrical progression; and, erec[ting] the perpendicular ordinates A H, B F, D G, E I, the [hy]perbolical spaces A H B F, B F G D, D G E I, wil[l be] equal*; and the parts A B, B D, D E, of the assympt[ote] will be in a continued decreasing geometrical progressi[on]; for, since, by construction, C A : C B : : C B : C D [: :] C D : C E, we shall have *dividendo* C A : C A – C B (A[B]) : : C B : C B – C D (B D) : : C D : C D – C E (D E); [and] *convertendo* C A : C B : C D : : A B : B D : D E. [We] may represent the lowering of the water therefore, or [the] evaporations, by the lines A B, A D, &c.; and the ti[mes] of these evaporations by the corresponding hyperbol[ical] spaces A H B F, A H D G, &c. This admitted, let [us] suppose A C to represent the quantity of water necess[ary] to saturate completely the body of air, in which the e[va]poration takes place; and the hyperbolical area A H E I represent any time, taken at pleasure: A E will repres[ent] the quantity of water evaporated during that time, and [E C] the difference between the quantity of water necessary [for] the complete saturation and the quantity of water eva[po]rated. Farther, if we draw *e i* parallel and infinitely n[ear] to the ordinate E I, E *e* will represent the evaporation t[hat] takes place during the fluxion of time represented by [the]

* F. Deschalles Cursus mathematicus, Sect. conic., Lib. prop. xli.

elementa

elementary parallelogram E I e i: but by the hypothesis the fluxion of time is constant; E e therefore is inversely proportional to E I, which is inversely proportional to C E; and consequently E e is directly proportional to C E: that is to say, the evaporation is at each instant proportional to the difference between the quantity of water necessary to saturate completely the body of air in which the evaporation takes place, and the quantity of water actually evaporated and suspended in that air; or, in other words, the evaporation is proportional to the excess of the moisture of the air at the point of saturation over the present humidity. This is the general law, which evaporation follows with respect to the humidity of the air.

Tends to confirm the opinion that evaporation is a true solution.

This law appears to confirm the opinion of Muschembrœck * and Leroy †, that the evaporation of water is nothing but a solution of this fluid in the ambient air; for the law just announced must take place generally in all solutions. In fact, if the menstruum did not exert on the body to be dissolved, in every portion of time, an action proportionate to the quantity that remained to be dissolved to produce complete saturation, but a greater or less action, it would follow, that, in the first case, when the menstruum is completely saturated it would still retain a part of its solvent action, which would remain without effect; and in the second, that the effect would be greater than its cause, which is equally absurd.

Attempt to find the quantity of water that would saturate air at a given temperature.

The application of this law supposes a knowledge of the quantity of water necessary to saturate completely a given quantity of air at a given temperature. To endeavour to accomplish this object, I repeated the elegant experiments of Mr. de Saussure ‡: but not having a large globe, I could only employ glass jars, the apertures of which I closed with a plate of metal cemented all round. Notwithstanding the imperfection of this apparatus, I had the satisfaction to obtain the same result as that celebrated philosopher; namely, that it required about 10 grains of water to saturate completely a cubic foot of air at the temperature

* Diss. Phys. Leyden, 1751, vol. II, p. 721.

† Mém. Acad. 1751, p. 484 and foll.

‡ Essais sur l'Hygromètrie, N. 97 and fol.

of

of 15° [65·75° F.]. I repeated the same experiments with air at the temperature of 20° [77° F.] and 5° [43·25° F.], and I found, that it required in the first case about 16·75 grs., and in the second 4·5. These three quantities are nearly in the same ratio as the evaporations under these degrees of heat given in the table inserted in this Journal*; and it is obvious, that it could not be otherwise.

Humidity of saturated air at 32°.

From this principle we have the proportion 17·1 : 4·4 : : 10 grs : 2·6 grs, the weight of the quantity of water contained by a cubic foot of air at the temperature of melting ice, when completely saturated. The weight of a cubic inch of water being 373·5 grs, this is the weight of 12 cub. lines of water; which, divided by the bulk of the air, will give $\frac{12}{(144)^3}$ for the humidity of the air at the temperature of melting ice, when completely saturated with water.

Rule for finding the point of saturation at other temperatures.

By the same reasoning may be found the humidity of a cubic foot of air at the temperature X, and at the point of saturation; by means of the proportion

$$(2{\cdot}718)^{\frac{0}{11\cdot05}}(4{\cdot}4) : (2{\cdot}718)^{\frac{X}{11\cdot05}}(4.4) : : \frac{12}{(144)^3} : \frac{12{\cdot}(2{\cdot}718)^{\frac{X}{11\cdot05}}}{(144)^3}.$$

This 4th term expresses the humidity to which the evaporation in air perfectly dry, at the temperature X, is proportional.

If we call Z the number of cubic lines of water suspended in a cubic foot of air, the temperature of which is equally X, the humidity of this air will be expressed by $\frac{Z}{(144)^3}$, and the evaporation in this air, agreeably to what has been said, will be proportionate to

$$\frac{12(2{\cdot}781)^{\frac{X}{11\cdot05}}}{(144)^3} - \frac{Z}{(144)^3}.$$

* Journ. de Phys., vol. LXV, p. 451; or our Journ. vol. XXVII, p. 22.

Consequently

Consequently the evaporation in air perfectly dry is to the evaporation in air that contains Z cubic lines of water in the cubic foot, at the same temperature X, in the ratio of $(2{\cdot}718)^{\frac{X}{11{\cdot}05}}$ to $(2{\cdot}718)^{\frac{X}{11{\cdot}05}} - \frac{Z}{12}$.

I have concluded from various experiments, that, by increasing the linear factor of the formula of evaporation * one fifth, this formula would pretty accurately represent the evaporation in air perfectly dry. If we make this correction, and reduce to lines by multiplying by the proportion $\frac{12}{150}$, confining ourselves to two decimal places, this formula will become (A) . : . . $(2{\cdot}72)^{\frac{X}{11{\cdot}05}}$ (0·34 lines); consequently $\left((2{\cdot}72)^{\frac{X}{11{\cdot}05}} - \frac{Z}{12}\right)$ (0·34 lines) is the formula, that gives the value of the evaporation, or lowering of the surface, of water expressed in lines, that takes place in twenty-four hours in air at the temperature of X degrees of De Luc's thermometer, and which contains Z cubic lines of water in the cubic foot. The calculation of the last formula is very simple; since it is sufficient, to deduct from the quantity calculated by the formula (A) the quantity Z (0·03 of a line); to which is reduced the effect of the humidity of the air.

Formula for calculating evaporation at any temperature and humidity of the air.

To facilitate the application of this formula, nothing is requisite but a more convenient and speedy mode of determining the number of cubic lines of water diffused in a cubic foot of air than that of drying this air by potash or quicklime, and then finding the increase in weight of the latter. I have sought this, which would lead, as has been seen, to the discovery of a true hygrometer; but my endeavours have not been more successful, than those of the celebrated natural philosophers, who have paid attention to the same subject.

Desideratum.

The imperfection of these researches has been the reason that I have so long deferred publishing them. I was still in

* Journ. de Phys. vol. LXV, p. 452: or Journ. vol. XXVII, p. 23.

 hopes

hopes of rendering them more perfect; but not being able to procure the instruments necessary for this purpose, I have resolved to communicate them to the learned, in hopes that these feeble attempts might perhaps induce them to turn their eyes toward this interesting subject, and give us at length a true theory of evaporation. *His principiis via ad majora sternitur* *.

Experiments made in the open air.

I had intended to add to this paper the experiments on a large scale, which I made on evaporation with cylindrical vessels full of water, the apertures of which were from three inches to eleven in diameter, and the height of which varied from eleven inches to eighteen. These vessels were placed in the open air in my garden, and buried within three lines of their apertures, at a little distance from each other. It was from these experiments I inferred evaporation to be proportionate to the surface of the water in contact with the air. I would also publish a journal kept for several months, to compare the evaporation that took place in the open air, and in a large vessel, with my formula, would it not occupy too much valuable room. I mention it, however, to show, that I have not always operated on confined air.

IV.

Remarks on the Construction of Fowlingpieces, pointing out Methods, by which they may be made to throw Shot very close, and the contrary. In a Letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

Improvement in the breech of a fowlingpiece.

THE following circumstance led me to take into consideration the construction of fowlingpieces, and to make what I conceive to be a useful improvement in that part, which is called the *breech*. Meeting by chance with an old foreign made gun barrel, of a construction that accorded much with my fancy, I purchased it, and determined to have it fitted up (first having tried it at a mark two or three times).

* Is. Newtoni Tract. de Quadrat. Curvarum, ad calcem.

I then

I then unscrewed the breech, or plug, which closes the hind part of the barrel, and ordered another, the form of which may easily be understood by referring to the annexed representation, Pl. ix, fig. 1. Now, after I had been at considerable trouble, and much pains, for I was determined to have it fitted up under my own inspection, I was very much disappointed in not being able to kill any thing with it, if at a greater distance than about 25 yards, in consequence of the shot being too much scattered. I varied the charge several times to shoot at a mark, but could by no means satisfy myself.

The bottom made concave.

This caused the shot to scatter.

After some time I took out the breech, and filed down the edges of the hollow part to the touch-hole, and fitted it up in the form of a common breech. I now found I had improved the killing quality of my gun, and had got pretty much out of conceit with the concave form of breeching guns. However it had this effect; since, thought I, the form I have just described, impairs the shooting quality of a fowlingpiece, there must be some *contrary* means of improving it; and accordingly I had one made of a form which will be easily understood, by referring to figure 2.

The bottom being made flat, the gun performed better.

Since this last improvement, I can with more certainty, (and I speak within compass) bring down a bird at the distance of about 60 or even 70 yards, than I could when I made use of the breech fig. 1, at the distance of 20 yards.

An improvement in the construction.

E, in fig. 2, is a strong iron or steel peg, standing out of the common breech, up the centre of the barrel, about an inch, (the thickness must be determined by the bore of the barrel), so as to contain the charge of powder round the peg; the wadding of the powder hereby resting upon the top of it, so as to prevent the powder being hard rammed. This not only keeps the grains of powder from being crushed by the ram-rod, but the impulsive force of the newly liberated air, on firing the powder, being removed from the central part of the charge of shot, has an opposite effect to that of the breech, fig. 1, viz. that of concentrating instead of dispersing the shot.

Its effect.

I am, Sir, your humble servant,

Bradford, Yorkshire, July the 7th, 1812. E. G.

the patient; if to obviate these inconveniences the main spring be made very strong, it is then, from its confined situation, exceeding liable to break; and if this does not happen, another inconvenience is produced, viz. from its very great strength it is scarce possible for any person to cock and discharge it with the requisite ease.

Improvement suggested.

It was therefore suggested, that, if two rows of lancets could be made to move in contrary directions, these, by keeping the skin equally stretched, would form clean incisions with much less force than in the former method: The scarificator A, marked No. 1, was therefore constructed, and first used early in 1802; it is accompanied by its working model B; this instrument at first contained the twelve long-edged spiral lancets C.

Defects of this construction.

This instrument immediately showed its theory to be good, but it had its faults; the incisions were too long when of the necessary depth; from the complex nature of the two racks, &c., and from the confined situation to which they were restricted, they could not be placed in the most favourable position for motion, and were therefore liable to be out of order.

These defects removed.

These and many other objections were altered or removed in various ways, which at length terminated in the construction of the instrument D, marked No. 2, also containing twelve lancets, combining every advantage and improvement suggested by experience and reflection. This instrument admits of two main springs, but from the manner in which the racks work in each other, and in their respective pinions, they in effect become one, but maintain the advantage of being made more slight, and consequently admitting a greater extent of motion than a single stiff spring can possibly accomplish; beside which they are not so liable to break; and should this happen to one, the instrument would not be useless, for I believe that one of these springs would be strong enough for all ordinary purposes, as incisions are effected with much less force when the lancets diverge; but combined they never have shown the least disposition to stop, however deep it might be necessary to set them, or strong the integuments to which they were applied; and consequently attended with greater ease to the patient. On

ingenious and respectable medical gentleman *, who speaks very satisfactorily of its performance, and which does not possess the advantage of the lancets withdrawing from the wounds, they being removed with the instrument, I thought, that should mine be a mistaken opinion, and that some real advantage attends this method, it occurred to me that submitting the instrument to the Society would be applying it to its best use; as from its possessing the property of the lancets withdrawing themselves, it might suggest some ideas for farther improvement.

I am, sir,

Yours obediently,

JOHN FULLER.

Description of the Engravings of Mr. JOHN FULLER'S *scarificator, Plate IX.*

Explanation of the plate.

Figs. 3, 4, 5, and 6, are sections of this instrument, taken in different positions, to explain its interior mechanism; fig. 3, is a plan of the lancets, the top of the box being removed to show them; fig. 4, is a section through the centre of the box; and fig. 6, the same, but taken in the opposite direction.

The lancets *a a*, 4, 5, and 6, are fixed upon two small arbors mounted parallel to each other across the box; the lancets are so arranged on the arbors, that those upon one arbor are placed in the intervals between the lancets fixed upon the other; the arbors are placed near the top of the box, and the lancets act through clefts cut in the lid, (as shown in figs. 4, and 6,) when the arbors are turned round; this is performed by a pinion upon each arbor, receiving motion from two toothed sectors, A B, fig. 5, which are also caused to act together by the teeth on their edges; they are fixed upon two parallel spindles C D, which extend across the box; the sector A, has a lever or handle attached to it, and coming through the bottom of the box, and by pulling this, the scarificator is wound up ready for action. The power is given by two horseshoe springs, *e e*, figs. 3 and 6, one end of each is screwed upon the bottom of the box, and the other acts in a notch *f f*, in each sector, so as to press

* See Journ. vol. xxvii, p. 124.

those

VI.

On a Case of nervous Affection cured by Pressure of the Carotids; with some physiological Remarks. By C. H. PARRY, *M. D. F. R. S.* *

OBSERVING that the Royal Society, of which I have the honour to be a member, occasionally receives communications illustrative of the laws of animal life, which are indeed the most important branch of physics, I take the liberty of calling their attention to a case, confirming a principle which I long ago published, and which I believe had never till then been remarked by pathologists.

Laws of animal life the most important in physics.

About the year 1786, I began to attend a young lady, who laboured under repeated and violent attacks, either of head-ach, vertigo, mania, dyspnœa, convulsions, or other symptoms, usually denominated nervous. This case I described at large to the Medical Society of London, who published it in their Memoirs, in the year 1788. Long meditation on the circumstances of the case led me to conclude, that all the symptoms arose from a violent impulse of blood into the vessels of the brain; whence I inferred, that as the chief canals conveying this blood were the carotid arteries, it might perhaps be possible to intercept a considerable part of it so impelled, and thus remove those symptoms, which were the supposed effect of that inordinate influx. With this view, I compressed with my thumb one or both carotids, and uniformly found all the symptoms removed by that process. Those circumstances of rapidity or intensity of thought, which constituted delirium, immediately ceased, and gave place to other trains of a healthy kind; head-ach and vertigo were removed, and a stop was put to convulsions, which the united strength of three or four attendants had before been insufficient to counteract.

Nervous affections suspended by compressing the carotid arteries.

That this extraordinary effect was not that of mere pressure, operating as a sort of counteracting stimulus, was evident: for the salutary effect was exactly proportioned to the actual pressure of the carotid itself, and did not take place at all, if, in consequence of a wrong direction, either

* Phil. Trans. for 1811, p. 89.

to

to the right or left, the carotid escaped the effects of the operation.

Mode in which it acts.

This view of the order of phenomena was, in reality, very conformable to the known laws of the animal œconomy. It is admitted, that a certain momentum of the circulating blood in the brain is necessary to the due performance of the functions of that organ. Reduce the momentum, and you not only impair those functions, but, if the reduction go to a certain degree, you bring on syncope, in which they are for a time suspended. On the other hand, in nervous affections, the sensibility and other functions of the brain are unduly increased; and what can be more natural than to attribute this effect to the contrary cause, or excessive momentum in the vessels of the brain? If, however, this analogical reasoning has any force in ascertaining the principle, I must acknowledge, that it did not occur to me till twenty years afterward, when a great number of direct experiments had appeared to me clearly to demonstrate the fact.

From various cases of this kind, I beg leave to select one which occurred to me in the month of January, 1805.

Case of nervous affection.

Mrs. T. aged 51, two years and a half beyond a certain critical period of female life, a widow, mother of two children, thin, and of a middle size, had been habitually free from gout, rheumatism, hæmorrhoids, eruptions, and all other disorders, except those usually called nervous, and occasional colds; one of which, about two years and a half before, had been accompanied with considerable cough, and had still left some shortness of breathing, affecting her only when she used strong muscular exertion, as in walking up stairs, or up hill.

In February 1803, after sitting for a considerable time in a room without a fire, in very severe weather, she was so much chilled as to feel, according to her own expression, "as if her blood within was cold." In order to warm herself, she walked briskly for a considerable time about the house, but ineffectually. The coldness continued for several hours, during which she was seized with a numbness or sleepiness of her left side, together with a momentary deafness, but no privation or hebetude of the other senses, or pain or giddiness of the head. After the deafness had subsided,

sided, she became preternaturally sensible to sound in the ear of the affected side, and felt a sort of rushing or tingling in the fingers of the left hand, which led her to conclude, that "the blood went too forcibly there." Case of nervous affection.

Though the coldness went off, what she called numbness still continued, but without the least diminution of the power of motion in the side affected. In about six weeks, the numbness extended itself to the right side.

Among various ineffectual remedies for these complaints, blisters were applied to the back, and the inside of the left arm above the elbow. The former drew well. The latter inflamed without discharging: so that a poultice of bread and milk was put on the blistered part. After this period, the muscles of the humerus began to feel as if contracted and stiff: and these sensations gradually spread themselves to the neck and head, and all across the body, so as to make it uncomfortable for her to lie on either side, though there was no inability of motion.

She now began to be affected with violent occasional flushings of her face and head, which occurred even while her feet and legs were cold, together with a rushing noise in the back of the head, especially in hot weather, or from any of those causes, which usually produce the feelings of heat.

It is difficult to give intelligible names to sensations of a new and uncommon kind. That, which this lady denominated numbness, diminished neither the motion nor the sensibility of the parts affected. It was more a perception of tightness and constriction, in which the susceptibility of feeling in the parts was in fact increased; and the skin of the extremities was so tender, that the cold air produced a sense of uneasiness, the finest flannel or worsted felt disagreeably coarse, and the attempt to stick a pin with her fingers caused intolerable pain.

In the month of September 1803, not long after the application of the blisters, she experienced in certain parts of the left arm and thigh that sensation of twitching, which is vulgarly called the "life blood," and which soon extended itself to the right side. Shortly afterward, she began to perceive an actual vibration or starting up of certain portions

effect might perhaps be produced, by compressing the carotid of the side affected. The event was exactly conformable to my expectation. Strong pressure on the right carotid uniformly stopped all the vibrations, while that on the left had no apparent influence. I may add, that these experiments were afterward, at my request, repeated on this lady in London by Dr. Baillie, and, as he informed me in a letter, with a similar result.

It is perfectly well known to many of the learned members of this Society, that irritations of the brain, when of moderate force, usually exhibit their effects on the nerves or muscles of the opposite side of the body; and in the case before us, it is difficult to understand how the suspension of these automatic motions could have been produced by this pressure of the opposite carotid, in any other way than by the interruption of the excessive flow of blood through a vessel morbidly dilated; in consequence of which interruption, the undue irritation of the brain was removed, and the muscular fibres permitted to resume their usual state of rest.

Undue impulse of blood on the brain the common cause of spasmodic or nervous affection.

From these and many other similar facts, I am disposed to conclude, that irritation of the brain, from undue impulse of blood, is the common though not the only cause of spasmodic and nervous affections; and I can with the most precise regard to truth add, that a mode of practice, conformable to this principle, has enabled me, during more than twenty years, to cure a vast number of such maladies, which had resisted the usual means.

An investigation of all the modifications of the principle itself, and of its numerous relations to therapeutics, would be inconsistent with the views of the Royal Society, and must be reserved for another place.

Bath, *Dec.* 8, 1810.

VII.

A concise View of the Theory respecting Vegetation, lately advanced in the Philosophical Transactions, illustrated in the Culture of the Melon. By T. A. KNIGHT, Esq. *F. R. S., &c.**

Theory of vegetation exemplified in the melon.

THE Council of the Horticultural Society having desired that I would send them a general view of my theory on vegetable physiology, which has been published by the Royal Society, I have great pleasure in obeying their wishes; and conceiving, that I shall be able to render it more clear amd useful, by making it illustrative of the proper culture of some particular plant, and by referring the reader to the papers in the Philosophical Transactions for evidence in support of the circumstances stated, I have for this purpose chosen the *melon*.

The seed.

A seed, exclusive of its seed-coats, consists of one or more cotyledons, a plumule or bud, and the caudex or stem of the future plant, which has generally, though erroneously, been called its radicle †. In these organs, but principally in the cotyledons, is deposited as much of the concrete sap of the parent plant, as is sufficient to feed its offspring, till that has attached itself to the soil, and become capable of absorbing and assimilating new matter.

The plumule.

The plumule differs from the buds of the parent plant in possessing a new and independent life, and thence in assuming, in its subsequent growth, different habits from those of the parent plant. The organizable matter, which is given by the parent to the offspring in this case, probably exists in the cotyledons of the seed, in the same state as it exists in the alburnum of trees; and like that, it apparently undergoes considerable changes before it becomes the true circulating fluid of the plant: in some it becomes saccharine, in others acrid and bitter, during germination ‡. In this process the vital fluid is drawn from the cotyledons into the caudex of the plumule or bud, through vessels which cor-

Caudex.

* Hort. Trans. vol. I, p. 217. Journ. vol. XXV, p. 118.

† Phil. Trans. 1809, p. 169:

‡ Phil. Trans. 1805.

respond

respond with those of the bark of the future tree, and are indeed perfect cortical vessels *. From the point of the caudex springs the first root, which, at this period, consists wholly of bark and medulla, without any alburnous or woody matter; and, if uninterrupted by any opposing body, it descends in a straight line towards the centre of the Earth, in whatever position the seed has been placed, provided it has been permitted to vegetate at rest †. First root.

Soon after the first root has been emitted, the caudex elongates, and taking a direction diametrically opposite to that of the root, it raises, in a great many kinds of plants, the cotyledons out of the soil, which then become the seminal leaves of the young plant ‡. During this period the young plant derives nutriment almost wholly from the cotyledons or seed-leaves, and if those be destroyed, it perishes. Gravitation by operating on bodies differently organized, and of different modes of growth, appears at once the cause why, in the preceding case, the root descends, and why the elongated plumule ascends §. Lengthening of the caudex.

The bark of the root now begins to execute its office of depositing alburnous or woody matter; and as soon as this is formed, the sap, which had hitherto descended only through the cortical vessels, begins to ascend through the alburnum. Bark of the root.
The plumule in consequence elongates, its leaves enlarge and unfold, and a set of vessels, which did not exist in the root, are now brought into action. These, which I have called the central vessels, surround the medulla; and, between it and the bark, form a circle, upon which the alburnum is deposited, by the bark, in the form of wedges, or like the stones of an arch ||. Through these vessels, which diverge into the leaf stalks, the sap ascends, and is dispersed through the vessels, and parenchymatous substance of the leaf; and in this organ the fluid, recently absorbed from the soil, becomes converted into the true sap or blood of the plant: and as this fluid, during germination, descended from the New set of vessels. True sap.

* Ibid. 1809: Journ. vol. XXV, p. 18.

† Phil. Trans. 1809, 1st part, p. 170: Journ. vol. XXV, p. 119.

‡ Phil. Trans. 1806.

§ Phil. Trans. 1st part, 1806. p. 4: Journ. vol. XIV, p. 409.

|| Phil. Trans. 1801, plate 27th.

cotyledons

As soon as the plant has attained its age of puberty, a portion of its sap is expended in the production of blossoms and fruit. These originate from, and are fed by central vessels, apparently similar to those of the succulent annual shoot and leaf stalk, and which probably convey a similar fluid; for a bunch of grapes grew and ripened, when grafted upon a leaf stalk; and a succulent young shoot of the vine, under the same circumstances, acquired a growth of many feet *.

Blossoms and fruit.

The fruit, or seed-vessel, appears to be generated wholly by the prepared sap of the plant, and its chief office to be that of adapting the fluids, which ascend into it, to afford proper nutriment to the seeds it contains. I proceed to offer some observations upon the proper culture of the melon †.

Fruit.

There is not, I believe, any species of fruit at present cultivated in the gardens of this country, which so rarely acquires the greatest degree of perfection, which it is capable of acquiring in our climate, as the melon. It is generally found so defective both in richness and flavour, that it ill repays the expense and trouble of its culture; and my own gardener, though not defective in skill or attention, had generally so little success, that I had given him orders not to plant melons again. Attending, however, after my orders were given, more closely to his mode of culture, and to that of other gardeners in my neighbourhood, I thought I saw sufficient cause for the want of flavour in the fruit, in the want of efficient foliage; and appealing to experiment, I have had ample reason to think my opinions well founded.

Melons not brought to due perfection,

from the want of efficient foliage.

The leaves of the melon, as of every other plant, naturally arrange themselves so as to present, with the utmost advantage, their upper surfaces to the light; and if by any means the position of the plant is changed, the leaves, as long as they are young and vigorous, make efforts to regain their proper position. But the extended branches of the melon plant, particularly under glass, are slender and feeble; its

Causes of this.

* Phil. Trans. 1803 and 1804; Journ. vol. X, p. 293.

† Ibid. 1801.

 leaves

more foliage cannot be exposed to the light. No part of the full grown leaves should ever be destroyed before the fruit is gathered, unless they injure each other, by being too much crowded together; for each leaf, when full grown, however distant from the fruit, and growing on a distinct branch of the plant, still contributes to its support; and hence it arises, that when a plant has as great a number of growing fruit upon part of its branches, as it is capable of feeding, the blossoms upon other branches, which extend in an opposite direction, prove abortive.

Importance of the leaves.

The variety of melon, which I exclusively cultivate, is little known in this country, and was imported from Salonica by Mr. J. Hawkins. It's form is nearly spherical, when the fruit is most perfect, and without any depressions upon its surface: its colour approaching to that of gold, and its flesh perfectly white. It requires a much greater state of maturity than any other variety of its species, and continues to improve in flavour and richness, till it becomes externally soft, and betrays some symptoms of incipient decay. The consistence of its flesh is then nearly that of a water melon, and it is so sweet, that few will think it improved by the addition of sugar. The weight of a good melon of this variety is about seven pounds. I send some seeds of it to be distributed amongst such members of the Horticultural Society, as may wish to receive them.

The variety cultivated.

VIII.

Some Remarks on Pruning and Draining standard Apple and Pear Trees. By Mr. JOHN MAHER, *F. H. S.**

WE often see apple and pear trees, both in gardens and orchards, not only crowded too closely together, but so loaded with their own branches, that very little fruit is produced; and that which is produced is rendered greatly inferior in size and flavour to what it would be under different management.

Apple and pear trees commonly too thick and crowded.

* Trans. of the Hort. Soc. vol. I, p. 236.

espalier, or of stakes, or of training and tying down the branches is incurred: 3dly, The crop of fruit is not only improved in size and flavour by having so much sun and air, but it is more easily gathered, and suffers much less from the autumnal winds; for branches in this direction are more pliable, and bend more easily to the storm; and as a proof how much may be done by art if necessary, the branches of a Lombardy poplar accidentally left in my master's orchard, after being loaded with clay balls, became as pendulous as those of the weeping willow *.

Branches of a poplar rendere[d] pendulous.

I have only to add, that most of the specimens of apples and pears produced at our meeting in November and December last by me, and honoured with the encomiums of some of the best judges present, grew upon trees kept low and open in this method.

The fruit excellent.

IX.

On the Advantages of employing Vegetable Matter as Manure in a fresh State. By T. A. Knight, *Esq. F. R. S., Pres. H. S.* †

Writers upon agriculture, both in ancient and modern times, have dwelt much upon the advantages of collecting large quantities of vegetable matter to form manures; while scarcely any thing has been written upon the state of decomposition, in which decaying vegetable substances can be employed, most advantageously, to afford food to living plants. Both the farmer and gardener, till lately, thought that such manures ought not to be deposited in the soil till putrefaction had nearly destroyed all organic texture; and this opinion is, perhaps, still entertained by a majority of gardeners: it is, however, wholly unfounded. Carnivorous animals, it is well known, receive most nutriment from the

Vegetable substances best for manure when fresh.

* Our President has shown, in the Philosophical Transactions of 1806, the extensive influence of gravitation upon the motion of the sap of plants; and his experiments perfectly support the author's conclusions.—*Secr.* See Journal, vol. XIV, p. 409.

† Trans of the Hort. Soc. vol. I, p. 248.

flesh

flesh of other animals, when they obtain it most nearly in the state in which it exists as part of a living body; and the experiments, I shall proceed to state, afford evidence of considerable weight, that many vegetable substances are best calculated to reassume an organic living state, when they are least changed and decomposed by putrefaction.

roof of this in seedling lum, I had been engaged in the year 1810, in some experiments, from which I hoped to obtain new varieties of the plum; but one only of the blossoms, upon which I had operated, escaped the excessive severity of the frost in the spring. The seed, which this afforded, having been preserved in mould during the winter, was, in March, placed in a small garden-pot, which was nearly filled with the living leaves and roots of grasses, mixed with a small quantity of earth; and this was sufficiently covered with a layer of mould, which contained the roots only of grasses, to prevent, in a great measure, the growth of the plants which were buried. The pot, which contained about one sixteenth of a square foot of mould and living vegetable matter, was placed under glass, but without artificial heat, and the plant appeared above the soil in the end of April. It was three times, during the summer, removed into a larger pot, and each time supplied with the same matter to feed upon; and in the end of October its roots occupied about the space of one third of a square foot, its height above the surface of the mould being then nine feet seven inches.

nd in potatoes. In the beginning of June a small piece of ground was planted with potatoes of an early variety, and in some rows green fern, and in others nettles, were employed instead of other manure; and, subsequently, as the early potatoes were taken up for use, their tops were buried in rows in the same manner, and potatoes of the preceding year were placed upon them, and covered in the usual way. The days being then long, the ground warm, and the decomposing green leaves and stems affording abundant moisture, the plants acquired their full growth in an unusually short time, nd afforded an abundant produce; and the remaining part of the summer proved more than sufficient to mature potatoes of any early variety.

Hint to the The market gardener may, probably, employ the tops

tops of his early potatoes, and other green vegetable substances in this way, with much advantage. gardener.

In the preceding experiments the plum-stone was placed to vegetate in the turf of the alluvial soil of a meadow, and the potatoes grew in ground which, though not rich, was not poor: and, therefore, some objections may be made to the conclusions I am disposed to draw in favour of recent vegetable substances as manures. The following experiment is, I think, decisive. Possible objections to the preceding experiments.

I received, from a neighbouring farmer, a field naturally barren, and so much exhausted by ill management, that the two preceding crops had not returned a quantity of corn equal to that which had been sowed upon it. An adjoining plantation afforded me a large quantity of fern, which I proposed to employ as manure for a crop of turnips. This was cut between the tenth and twentieth of June; but as the small cotyledons of the turnip-seed afford little to feed the young plant; and as the soil, owing to its extreme poverty, could not afford much nutriment; I thought it necessary to place the fern a few days in a heap, to ferment sufficiently to destroy life in it, and to produce an exudation of its juices; and it was then committed in rows to the soil, and the turnip-seed deposited, with a drilling machine over it. A decisive one.

Some adjoining rows were manured with the black vegetable mould obtained from the site of an old wood pile, mixed with the slender branches of trees in every stage of decomposition, the quantity placed in each row appearing to me to exceed, more than four times, the amount of the vegetable mould, which the green fern, if equally decomposed, would have yielded. The crop succeeded in both cases; but the plants upon the green fern grew with greatly more rapidity than the others, and even than those which had been manured with the produce of my fold and stable-yard, and were distinguishable, in the autumn, from the plants in every other part of the field, by the deeper shade of their foliage.

I had made, in preceding years, many similar experiments with small trees (particularly those of the mulberry when bearing fruit in pots) with similar results: but I think it unnecessary Similar previous experiments.

astringent, and bitter taste: it precipitated gelatine copiously, and adhered strongly to animal substances, which it dyed of a saffron colour: it was more soluble in hot water than in cold: it was dissolved by potash, and this compound at the expiration of some days had deposited a small quantity of detonating matter. It was readily soluble in concentrated nitric acid, and in alcohol. I analysed it in the following manner.

4. *a.* I took 40 parts, that I had dried in a capsule with a gentle heat, boiled them in distilled water, and added in three portions 30 parts of carbonat of lead. Effervesence took place. After an hour's boiling, I filtered. Analysis of it.

b. A pulverulent matter, of the colour of bistre, remained on the paper, which, being washed with cold water, acidulated with sulphuric acid, yielded sulphate of lead, a liquor containing some *resin*, and a small quantity of the two *amers*.

c. The filtered liquor (*a*) was decomposed by sulphuric acid. The sulphate of lead being separated, the liquor was concentrated: and a little oily matter was deposited, which had either escaped the action of the carbonate of lead, or was regenerated during the evaporation. The liquor separated from the oily matter, and concentrated farther, left a thick, very bitter, and astringent matter, that precipitated gelatine. This was divided into two portions, No. 1, and No. 2.

d. No. 1 was mixed with potash, which coagulated it almost into a mass. It was then diluted with cold water, and filtered. On the filter was left a deep yellow powder, not crystallizable, and susceptible of detonation, but less so than the compound of amer at a maximum and potash. The aqueous solution of this powder reddened sulphate of iron at a maximum, and precipitated gelatine, when the alkali had been previously saturated with an acid. This yellow powder was a compound of potash, amer at a minimum, and amer at a maximum: for, having decomposed it by muriatic acid, I obtained by spontaneous evaporation, 1st, crystals of amer at a minimum retaining a little amer at a maximum; 2dly, a mother water, which, being saturated with potash, yielded a detonating substance in small yellow needles,

needles, altogether resembling that formed with amer at a maximum.

The liquor from which the yellow detonating powder was separated contained some resin, with the addition of a small quantity of amer at a minimum.

e. No. 2 was treated with concentrated nitric acid. A few small globules of an oily appearance were formed. It was boiled, evaporated to dryness, dissolved in water, and saturated with potash. The compound of amer at a maximum with potash was obtained crystallized. The mother water yielded on evaporation fresh detonating acicular crystals, of a deeper yellow than the first from their retaining a little resin.

Inferences from this. From these experiments I concluded,

1st, that the substance of an oily appearance is formed of resin, amer at a minimum for the greater part, amer at a maximum, and perhaps nitric acid combined with all three, and contributing to their fluidity: 2dly, that amer at a minimum is capable of combining with amer at a maximum *, and forming a detonating compound with potash.

Rationale of the analysis. 5. The analysis of the matter of an oily appearance is founded on the difference of solubility of amers and resin, and on that of their compounds with the oxide of lead. Thus, when carbonate of lead is boiled with the oily matter, the amers and a small quantity of resin combine with the metallic oxide, and form compounds soluble in water: and as soon as the greater part of the resin is freed from the amers, it is much less soluble, and consequently will separate; and its separation is farther promoted by its combining with a little oxide of lead. The resin, from its affinity for the amers, carries down a small quantity with it. When the soluble compound of amers with lead is decomposed by sulphuric acid, the two amers combine together, so that they jointly form a compound of little solubility with potash; and when this compound is treated with muriatic acid,

* The compound of the two amers has appeared to me capable of assuming the form of globules of an oily appearance at a temperament of + 60° [140° F.] Perhaps the nitric acid contributes to give it this state.

the

ıe amers separate from each other in the order of their espective solubilities.

6. I do not consider the substance of an oily appearance ; uniform in the proportions of its immediate principles. ometimes the amer at a minimum is in small quantity: ɔmetimes it predominates over the others. The same may e said of the amer at a maximum, and of the resin. This is ıe reason of the differences observed in the colour, con-stence, and other properties of these compounds. The proportions of the immediate principles not constant.

7. It remains for me to assign the reason why the com-ɔund of resin and amers precipitates gelatine much more ›piously than amer at a maximum. The fact appears to e owing to this, that the resin and amer at a minimum which have themselves an affinity for animal substances, but ɔt sufficient to precipitate gelatine) in combining with ner at a maximum diminish the solubility of the latter, so-dify it in some degree, and thus in all probability increase ıe capacity it has of forming with gelatine a compound ut little soluble. Why the compound precipitates gelatine.

§ II. *Bitter tanning substance formed with extract of rasil wood.*

8. In my first paper on brasil wood, I described a bitter ıbstance, produced by the action of nitric acid on the ex-act of this wood. I showed, that this substance did not ystallize, was acid, precipitated gelatine, and formed with otash small detonating crystals. At that time I considered ıis substance as a compound of amer, artificial tannin, and itric acid. My reasons were, 1st, its being fusible by eat, and forming detonating salts with salifiable bases, in ıe manner of Welther's amer: 2dly, its precipitating gela-ne in the manner of the tannin described by Mr. Hatchett, hile the amer of Welther, prepared with indigo, does not recipitate it; whence I inferred, that two different sub-tances were requisite, to form detonating compounds and pre-ipitate gelatine*: 3dly, its reddening litmus paper; because, ı treating extract of brasil with nitric acid, I had obtained another Tannin from extract of brasil. Supposition respecting its nature.

* The reason of my not obtaining a precipitate with Welther's mer and gelatine at that time was my employing solutions too di-ıte, or adding too much gelatine.

to have a greater tendency to solidity than the latter; and that the presence of a certain quantity of resin, with which it may be united, favours this precipitation.

§ III. *Tanning substance formed with aloes.*

11. On treating aloes after the mode of Mr. Braconnot, I obtained the matter he called aloetic acid *. Tannin from aloes.

This acid substance has a yellow colour; an acid, astringent, and bitter taste: projected on a hot iron, it emits a yellow smoke, is carbonized, and melts. Heated in the glass bulb it yields, 1st, water; 2d, carbonic acid; 3d, prussic acid; 4th, an inflammable gas, which I believe to be a mixture of oily hidrogen and gaseous oxide of carbon; 5th, nitrogen gas; 6th, charcoal.

Hence it appears to me, that the aloetic acid is a compound of nitric acid analogous to those I have already described. A similar compound.

This substance is but little soluble in water; and, though it is yellow, its solution is of a fine purple. I imagine the water acts in this case by weakening the action of the nitric acid on the vegetable matter with which it is combined. This purple solution becomes yellow on the addition of nitric acid, muriatic acid, &c. Its properties

It gives a purple colour to alcohol.

I boiled some in nitric acid at 50° [sp. gr. 1·5], and evaporated to dryness. On dissolving the residue in water, I obtained a red solution, inclining a little to fiery, because probably there remained a little excess of acid. It seems to me, therefore, that the nitric acid had not changed the nature of this substance. not altered by nitric acid.

With salifiable bases it forms purple compounds, which are detonating, as Mr. Braconnot first observed. Its compounds with the bases.

I have found, that it precipitates gelatine very well; and even that several of its soluble compounds precipitate it without the addition of an acid, which Welther's amer combined with potash never does, when it contains no resin. Precipitates gelatine.

12. Is the aloetic acid a compound of nitric acid and a substance arising from the decomposition of aloes? or a compound of nitric acid and the coloured principle of aloes Its nature.

* Journal, vol. xxvii, p. 365.

little

little or not at all changed? The colour of this substance leads me to incline to the latter opinion: yet I do not consider it as impossible, that, beside the acid and coloured principle, it may contain a portion of aloes with its nature changed; for the pretty copious production of oxalic acid in the treatment of aloes by nitric acid proves, that a part of the aloes is completely decomposed.

Hypotheses.

13. Perhaps it may be thought, that the amers of brasil and aloes are only compounds of amer at a maximum, nitric acid, and substances resulting from the more or less advanced decomposition of the articles with which they were formed. Without venturing to assert, that this opinion is absolutely false, it appears to me more natural at present, to consider these amers as two distinct species of amer at a maximum. Hence it follows, that resinous substances treated with nitric acid do not afford a homogeneous principle, that may be considered as a kind of artificial tannin. Besides, the following experiments will prove, that the faculty of precipitating gelatine belongs to substances of a very different nature, and in which the presence of amer at a maximum cannot be suspected.

Different substances precipitate gelatine.

Tannin from pitcoal.

Part II. *Tanning matter formed with carbonaceous substances.*

§ I. *With pitcoal.*

Mr. Hatchett's experiments on bitumens

1. Mr. Hatchett asserts, that several bitumens, as jet and asphaltum, are formed of charcoal and a resinous matter; and that, when nitric acid is digested on these compounds, the carbonaceous part dissolves, and the resinous part separates in the form of a yellow or orange-coloured mass. Mr. Hatchett applies this discovery to pitcoal; and says, that when this contains no resinous substance, which is the most common case according to him, it is completely dissolved by nitric acid, and converted into tannin; and that, on the contrary, when it contains a little resinous matter, this is not dissolved. The results I have obtained differ a little from those of Mr. Hatchett. Like him, by treating pitcoal with concentrated nitric acid, and reducing the liquor to a sirupy consistence, I obtained a thick, brown homogeneous liquid; but when this liquid was poured into water, a yel-

and coal.

The author's results somewhat different.

a yellow matter separated, which was much more abundant than what remained in solution, and had no property, that rendered it similar to resins. I obtained the same results with two varieties of pitcoal, yet I do not allow myself the least reflection on the labours of that celebrated English chemist; as I am too fully aware, that different modes of operating, and the different varieties of the bodies examined, are so many causes, that may produce a variation in the results. I shall proceed therefore to relate my own experiments, and deduce from them the conclusions, that appear to me most natural. Causes of difference in results.

2. The pitcoal I used was perfectly pure. 100 parts heated strongly in a platina crucible left 84 parts of coak. Pitcoal used.

I digested 100 parts of this coal, finely powdered, in 600 parts of nitric acid at 44° [sp. gr. 1·425]. An effervescence took place, with the evolution of nitrous vapour, &c. When the action diminished I increased the heat; and at the expiration of 24 hours I added 600 parts of nitric acid, and heated to boiling, taking care to pour back into the retort the acid that passed over into the receiver. Finally, when the matter appeared to be thoroughly attacked, I poured it out into a capsule, and evaporated gently to dryness. The residuum weighed 170 parts, consequently there was an increase of weight of seven tenths. The hot water, with which I washed it repeatedly, acquired a reddish brown colour, and an acid astringent taste, from dissolving the tannin of Mr. Hatchett. The yellow and little soluble matter, which I shall designate by the letter A, was not dissolved. Treated with nitric acid.

Art. I. *Examination of the tanning matter of Mr. Hatchett.*

3. I evaporated to dryness the washings of the pitcoal treated by nitric acid (2), redissolved the residuum in a small quantity of water, and thus separated a little of the matter A. The filtered liquor had an acid taste, with a little bitterness and astringency: it coagulated gelatine very well. To separate the tanning substance in a state of purity, I poured into it acetate of lead, till no more precipitate was thrown Examination of the artificial tannin.

thrown down. I then poured off the liquid, which was of a light yellow, and washed the precipitate with a great deal of water.

4. This precipitate, which was a compound of the tanning matter and oxide of lead, was thrown still wet into water acidulated with sulphuric acid. After boiling I left these substances to act on each other for twenty-four hours. At the expiration of this time I satisfied myself by means of barytes water and sulphuretted hidrogen, that there was neither sulphuric acid nor lead in the solution.

Its properties.

I filtered the liquor to separate the sulphate of lead, and evaporated it to dryness. A brown mass remained, which melted by heat, hardened on cooling, and afterward attracted moisture from the atmosphere. The aqueous solution of this substance reddened litmus, and formed a precipitate with gelatine, barytes water, and acetate of lead. The precipitates with the latter two were soluble in nitric acid; and melted when exposed to heat in a glass tube closed at one extremity, emitting an aromatic smell mixed with something of the prussic. When operating on the precipitate with lead, if the residuum were thrown on a paper while hot, it took fire like a pyrophorus. This combustion was produced by charcoal and metallic lead in a state of minute division. The residuum of the compound with barytes was very little pyrophoric.

Examination of it for nitric acid.

5. To ascertain whether any nitric acid were present in the tanning matter prepared by the preceding process, I introduced 5 dec. [7·7 grs] into the glass bulb, and heated them. The matter fused, because it contained a little humidity; and evolved with much impetuosity aqueous vapour, ammonia, carbonic acid, nitrous gas, &c. A coal remained, that emitted a strong smell of prussic acid.

Combination of it with sulphuric acid.

6. As I tried the preceding experiment several times, I found, that sulphuric acid was capable of combining with the tanning matter, when it separated it from oxide of lead; and that this compound, when it did not contain an excess of sulphuric acid, formed with barytes a precipitate soluble in nitric acid, and gave out sulphuric acid when heated. It seemed to me, that by boiling carbonate of lead with this compound dissolved in water, evaporating to dryness and redissolving

redissolving in water repeatedly, the oxide of lead united with the sulphuric acid, and a substance was obtained, which when heated no longer gave out any sensible quantity of sulphurous acid. This experiment I performed but once.

7. The liquor, from which the tanning matter had been separated by acetate of lead, had sulphuretted hidrogen passed through it; after which it was filtered, and evaporated to dryness. The residuum was dissolved in water, and potash was added to the solution. This produced a yellow precipitate of lime retaining some bitter matter. The liquor being filtered and concentrated yielded silky crystals, of a golden yellow colour, detonating, and resembling those formed by Welther's amer and potash. Proust had already observed, that a small quantity of this substance was formed, when pitcoal was treated with nitric acid at 40° [sp. gr. 1·396].

Amer found in the liquid from which the tannin had been separated.

Hence it follows, that the matter soluble in water is formed, 1st, of a substance that precipitates gelatine copiously, which is a compound of nitric acid, and carbonaceous matter; 2d, of a very small quantity of amer at a maximum. The acetate of lead forms with the first a compound insoluble in water, and with the second a compound soluble in it.

Nature of the compound.

Art. II. *Examination of the matter* A.

8. The matter A, after it had been several times washed, was of the colour of umber. It had a slightly acid taste; and reddened litmus paper on which it was moistened with a little water. Heated in a glass tube it melted, emitting a red light, and a smell of nitrous acid mixed with prussic. To destroy the supposition, that the nitrous acid might have arisen from the remains of the acid that escaped the waters of elutriation, I digested the matter A in water, filtered, and washed it repeatedly with fresh water. Of the substance thus washed I heated 2 dec. [3 grs] in the glass bulb. The matter fused, and gave out, 1st, water; 2d, nitrous vapour; 3d, carbonic acid; 4th, ammonia; 5th, some inflammable gas, which appeared to me a mixture of oily hidrogen and gaseous oxide of carbon, for it burned with a heavy white flame, and presently with a blue; 6th, nitrous gas;

Examination of the difficultly soluble matter.

gas; 7th, nitrogen gas *; 8th, prussic acid, sensible to the smell, but in too small quantity to afford prussian blue; 9th, charcoal.

9. The matter A digested with a small quantity of water coloured it red, and gave it the property of precipitating gelatine. The residuum boiled with fresh water was in part dissolved; and ultimately left a blackish substance, heavier than the matter A, and very slightly colouring water with which it was boiled. I believe it was nothing but the oxide of carbon described by Proust. To this I shall presently return.

10. The washings of the matter A were concentrated by a gentle heat. A substance was deposited, apparently very similar to A, and a very astringent matter remained in the concentrated liquor.

Separated into three substances.

Hence it follows, that water separated the matter A into three different substances: 1st, a black substance, nearly insoluble in water, which I shall call A^1: 2d, a substance soluble in water, but precipitable from it by evaporation, which I shall denote by A^2: 3d, a substance very soluble in water, A^3.

The insoluble substance examined.

11. A^1 was a little acid. 5 dec. [7·7 grs], heated in the glass bulb, melted, diffusing a red light, and giving out, 1st, water; 2d, carbonic acid; 3d, inflammable gas, burning white; 4th, nitrous gas; 5th, nitrogen gas; 6th, a little ammonia; 7th, a coal, that emitted a strong smell of prussic acid.

A compound of nitric acid and carbon.

Hence it is evident, that this substance, which possesses the properties ascribed by Proust to the oxide of carbon, is a compound of nitric acid and carbon: it differs from A^2 and A^3 only by containing less acid: and what appears to confirm this is, by boiling it in concentrated nitric acid it is totally dissolved; and, when water is poured into this solution, it throws down a yellow flocculent precipitate, exhibiting all the properties of the unwashed matter A. Hence I imagine, that, when the matter A is boiled in water, the portion

* With respect to this product see what I have said in the article of the decomposition of Welther's amer by heat in my paper on the amers from indigo. See Journ. vol. XXX, p. 351.

portion which does not dissolve cedes a part of its acid to that which dissolves; and, when the washings are afterward evaporated, a farther division is made of the acid between the substance A², which is precipitated, and A³, which remains in solution.

Nitric acid, at least in the proportion in which I employed it, could not convert A¹ into the tanning matter of Hatchett, which is very soluble in water. There is a portion of matter, however not separated by water from the nitric solution of A¹, which precipitates gelatine; but I cannot assert, that it is absolutely similar to the matter of Hatchett.

Attempt to convert it into tannin.

To find whether it were possible, to remove the nitric acid from A¹ without heating it, I digested it in a weak solution of neutral carbonate of potash. By the assistance of heat carbonic acid was evolved, and nearly the whole was dissolved.

The nitric acid not separable from it by carbonate of potash.

This solution was decomposed by sulphuric acid, which threw down a brown flocculent precipitate. The supernatant liquid was colourless. It was filtered: the slight excess of sulphuric acid contained in it was saturated with carbonate of potash: it was evaporated to dryness, and the residuum was treated with alcohol at 30° [sp. gr. 0·868], to dissolve the nitre, if it contained any; but none was found. The carbonate of potash therefore had taken no observable quantity of nitric acid from A¹.

The brown precipitate left on the filter was washed with hot water, till this gave no farther indication of sulphuric acid to the test of solution of barytes. At this period the water of elutriation was fawn-coloured, had a taste and smell slightly inclining to those of oak bark and roses, and did not perceptibly precipitate gelatine. On adding an acid, a little flocculent precipitate fell down.

After it is separated from potash by sulphuric acid rather more soluble,

If water dissolve more of A¹ that has been precipitated from potash by sulphuric acid, than of that which has not, I believe it depends on its being more minutely divided: for in that which I prepared with care I found no sensible quantity of sulphuric acid *, and its coal afforded only an atom of potash.

from being more minutely divided.

* Experiments I have since made lead me to think, that potash contributes to the solution of this substance in water.

5 dec. [7·7 grs] of A[1], which had been dissolved by the carbonate, and afterward precipitated by sulphuric acid, melted with heat, and afterward gave out carbonic acid gas, nitrous gas, &c., leaving a coal, that emitted a smell of prussic acid, and contained an atom of potash.

Examination of the matter precipitated by concentration.

12. A[2], which dissolved in the water of elutriation of A, and afterward fell down during its evaporation (10), was of a blackish brown colour. Treated with boiling water part was dissolved, and imparted to the water the property of coagulating gelatine. The solution yielded by evaporation a residuum, that melted, and evolved nitrous gas. The part but little soluble in water greatly resembled A[1]. It melted, and gave out nitrous gas, but in smaller quantity than the portion that had dissolved in the water. This indicates, that acid was transferred from the portion but little soluble to the other.

Examination of the very soluble substance.

13. A[3], which remained in solution after the concentration of the washings of A, and had been obtained by evaporating them, was fawn-coloured. Heated in the glass bulb it melted; yielded water, carbonic acid, nitrous gas, &c.; and left a coal, that emitted a very strong smell of carbonate of ammonia.

Its difference from artificial tannin.

This substance, which precipitated gelatine very well, differed from Hatchett's tannin (Art. I) in its alkaline solution being precipitable by acids, in its being consequently less soluble in water, and in its not melting by heat.

The three differ only in their proportions of nitric acid.

39. The matter A therefore is divisible by water into three portions, which differ from each other only by the quantity of nitric acid they contain, since by taking a portion of this acid from those that contain the most they are converted into those that contain the least; and by adding acid to those that have lost it, they are brought back to their former state.

§ II. *Tanning matter formed with fir charcoal.*

Artificial tannin from charcoal.

40. A hundred parts of fir charcoal, which had been calcined in a platina crucible, in a red heat, required for their solution in nitric acid more time and more acid than 100 parts of pitcoal. The solution of the charcoal was brown, and thick like a sirup. When water was added, a brown matter

matter separated, which I shall examine below. The liquor freed from this was evaporated to dryness. The residuum was black, a little astringent, and slightly acid. Heated in a glass tube, it did not melt, but an acid vapour was evolved. The greater part dissolved in distilled water. This solution precipitated gelatine, and many metallic salts. The precipitate formed with acetate of lead, being heated in a glass tube, left a coal mixed with metallic lead, which took fire, if thrown on paper while hot.

41. To obtain the tanning matter in a state of purity, I precipitated the solution by acetate of lead, and washed the precipitate, till the water that came off ceased to be coloured by sulphuretted hidrogen. I decomposed the precipitate, while yet wet, by sulphuric acid. The sulphate of lead was separated by the filter. With barytes and acetate of lead the liquor threw down a flocculent precipitate, soluble in an excess of nitric acid; which indicated, that it contained no sensible excess of sulphuric acid *. However, having evaporated to dryness, I obtained a brown, deliquescent residuum †, fusible by heat; which, being heated in the glass bulb, gave out carbonic acid gas, sulphurous acid gas, and other gasses insoluble in water, which I was unable to examine from the smallness of their quantity (for I operated only with 2 dec. [3 grs] of tanning matter); so that I know not whether any nitrous gas were among them. Purified, and examined.

42. Thus it appears, that, when the compound of tanning matter and lead is decomposed by sulphuric acid, the Sulphuric acid combines with the tanning matter.

* To find whether a liquid contain any excess of sulphuric acid, solution of barytes should be poured into it. If a precipitate form, try to dissolve it in pure nitric acid. If this dissolve it, the matters should be left to act on each other for 24 hours, and then see whether there be any precipitate. I have often observed that lead indicated no sulphuric acid, where barytes did perceptibly. The latter therefore is preferable as a test to the former. Barytes recommended as a test of sulphuric acid.

† I have found, that by heating this residuum a little strongly in contact with the air in a capsule white fumes of sulphuric acid were evolved; and the substance thus heated, being redissolved in water, gave indications of sulphuric acid, when tested with solution of barytes.

latter,

latter, if in excess, enters into combination. It is probable, that the sulphuric acid combines with the tanning matter, without expelling the nitric acid.

Earthy matter of the coal, and yellow bitter matter.

43. The liquor from which the tanning matter had been precipitated by acetate of lead having had sulphuretted hidrogen passed through it, and been afterward filtered, contained the earthy matters of the coal, and a yellow bitter matter, the nature of which I could not positively ascertain.

Brown matter insoluble in water.

44. I dissolved the brown matter I have mentioned (40) in nitric acid at 45° [sp. gr. 1·435], concentrated the solution, and afterward added water to it. This precipitated a yellowish substance, similar in appearance to the matter A, separated by water from a nitric solution of pitcoal: but the yellowish substance differs from A in being entirely soluble in boiling water, and in not being fusible by heat. I presume, that it differs from the portion soluble in water (41) only in containing less nitric acid, and perhaps more hidrogen.

XI.

Chemical Examination of the Husks of Walnuts. By Mr. Henry Braconnot, *Prof. of Nat. Hist. &c.**

Husks of walnuts used in dyeing.

The daily use of the husks of walnuts in the art of dyeing suggested to me the wish of making some experiments on them, to be enabled to form a more accurate judgment of their nature.

Soon changed, unless kept from air. Apparently a slow combustion effected.

When fresh the husk is interiorly white, but it becomes coloured very quickly, and ultimately passes to a dark brown. This is owing to the contact of the air; for, if it be immersed in water that has been boiled, it will keep some time without undergoing the least change. If it be placed in a jar filled with atmospheric air, the oxigen will soon be converted almost wholly into carbonic acid; the husk acquires a blackish colour; and no doubt there is also a production of water: so that the whole seems to indicate

* Ann. de Chim. vol. LXXIV, p. 303.

the

the phenomena of a slow combustion. Oximuriatic acid appears to have another kind of action on it; for, instead of blackening it, it causes it to assume a yellow colour. Nitric acid comports itself in the same manner.

Action of oximuriatic and nitric acid.

To proceed to the examination of the matters contained in the husk, I bruised a certain quantity in a marble mortar, expressed the juice, and filtered it. Some green feculæ remained on the paper, which soon changed to a deep brown by exposure to the air. This matter, washed and dried, was macerated in alcohol, which extracted from it the green resin common to most vegetables. The residuum insoluble in alcohol was still coloured, and felt smooth. A portion of it was diluted with weak nitric acid, which converted it into a thick substance, viscous, and soluble in water. In this solution alcohol occasioned a white flocculent precipitate. The same coloured residuum, being diluted with water to which a little potash was added, produced a bulky tremulous substance, of a deep red colour, and resembling the coagulum of blood. Lastly another portion of the same residuum was dissolved in boiling water, and formed starch. Hence it follows, that this substance, contained pretty abundantly in the husk, is starch contaminated by the colouring matter.

Expressed juice filtered.

Residuum examined.

The juice of the husk recently filtered is of an amber colour, and of an acrid and sour taste mixed with bitterness. This acrid principle appears extremely destructible, for the recent juice, left to itself some days, while it loses its yellow colour to assume a blackish brown where it has been in contact with the air, loses also its acrimony, and becomes decidedly acid: at the same time black pellicles form on its surface, which are soon replaced by others if removed. These pellicles, carefully collected and well washed, yielded on drying a black, brittle substance, of a shining, vitreous fracture, and pretty similar to asphaltum, or Jew's pitch, but burning without any apparent flame, in which it more resembled charcoal. This carbonaceous matter was dissolved in potash, and in this solution a flocculent precipitate was produced by acids. It may be obtained more readily by evaporating the juice of the husk with a gentle heat, and diluting the residuum with water. The liquid

Examination of the filtered juice.

liquid standing on the sediment is a pure and even agreeable acid; whence it follows, that the acrid and bitter principle has been entirely destroyed, being converted apparently into the black matter approaching the state of charcoal. The same extract gave out no acetous vapour with sulphuric acid, even heated: it contains therefore no acetic acid.

A compound of carbon and hidrogen dissolved in the juice, as in that of many plants. From what has been said we cannot but observe in the husk of the walnut, as in many herbaceous plants, a substance held in solution in its juices; and the hydrocarburet radical of which is more or less decomposable by the simple contact of air, which appears to cause a production of water, rendering the carbon predominant. It is obviously impossible to have a very accurate idea of a substance so little permanent: but it appears, that it is but slightly coloured in the vessels of plants; and that the action of the air or of caloric alters it greatly; causing it to pass by degrees to the state of extract, another principle badly defined, of little stability in respect to its element, and which seems rather the result of a decomposition, than a real product of living nature*.

Effects of tests on the juice. The juice of the husk examined by reagents exhibited the following effects.

Litmus. It strongly reddened infusion of litmus.

Gelatine. Solution of gelatine formed in it a slight precipitate, which must have been owing to tannin.

Extracts altered by keeping. * Having had an opportunity of examining some extract of rhus toxicodendron, that had been prepared several years before, I made the following observation. I applied some to the skin of an animal, and gave him some internally in pretty large doses, without his experiencing any troublesome consequences; while one drop from the stalk of the plant on the skin occasioned a tolerably extensive inflammation, terminating in an ulcer. Thus it appears, that the principles of plants condensed to the state of extract undergo an alteration, which continues progressive with time; and this must cause their action on the animal economy to vary greatly. They should be kept dry, and air excluded. Perhaps apothecaries may prevent this alteration in a certain degree, by enclosing their extracts, when perfectly dry, in vessels well stopped; for the moisture they contain, or have a tendency to absorb, does not contribute less to alter the feeble equilibrium of some of their elements, than the contact of air.

Sulphate

Sulphate of iron gave the juice so deep a green, that it appeared black. No precipitation took place, even on standing some time, in consequence of the free acid found in the mixture, which is capable of imparting a fine gray to wool or silk. Sulphate of iron.

Oxalate of ammonia indicated the presence of lime. Oxal. of ammonia.

Nitrate of barytes produced no signs of any sulphate. Nitr. of barytes,

Nitrate of silver acts on it in a manner well adapted to reveal the presence of the alterable hydrocarburet radical, for it produces a pretty copious precipitate, which quickly becomes coloured; while the silver resumes its metallic lustre from the action of the vegetable substance on the oxigen of the oxide. The precipitate is then no longer soluble but in part in nitric acid, and leaves charcoal as a residuum. and of silver.

Alkalis change the juice to a deep red, and form in it precipitates that contain lime. If after a certain time an acid be poured into the liquor, another flocculent sediment is produced, which dries, grows black, has a vitreous fracture, and resembles in its nature the pellicles, that are formed successively on the surface of the juice exposed to the air. Alkalis.

Acetate of lead occasioned in the juice a whitish, flocculent, very copious precipitate, which dissolved entirely in distilled vinegar. This precipitate, being decomposed by sulphuretted hidrogen, yielded a coloured liquor, of considerable sourness mixed with astringency, which produced a sediment with gelatine, and with acetate of lead a precipitate soluble in vinegar. This acid, being evaporated by a gentle heat, yielded small, ill-defined crystals, immersed in the uncrystallizable liquor. The whole was mixed with carbonate of lime; and after the mixture, which contained an excess of acid, had been heated, I filtered it. By evaporating I obtained a granular, coloured substance, formed by the union of a number of small acicular crystals. This salt, being treated with cold water, dissolved in it in part: and the solution, evaporated to dryness, left a brown varnished residuum, which comported itself like malate of lime, retaining some tannin, which then precipitated iron of a blackish blue. The portion of the calcareous salt that would Acetate of lead.

would not dissolve in cold water was treated with diluted sulpuric acid, which separated from it citric acid, still contaminated with malic.

Subacetate of lead.

The juice thus freed from part of the matters it held in solution was still coloured. Acetate of lead supersaturated with oxide produced in it another sediment, and rendered the supernatant liquor nearly colourless. This sediment yielded on analysis the same products as above; namely malic acid, colouring matter, and tannin, which had escaped the first precipitation in consequence of the presence of the acetic acid, that had become predominant in the liquor.

Examination of the magma.

The magma left after expression of the juice, after having been treated with alcohol, which extracted from it some green resinous matter, was heated with water till it boiled, to free it from the starch and the coloured matter it retained. When thus exhausted, it was digested with dilute nitric acid, which separated some phosphate and oxalate of lime, that had been precipitated from the acid liquor by ammonia. The means I employed to separate these two earthy salts, which are very frequently associated together in vegetables, are founded on the property distilled vinegar diluted with water has of dissolving phosphate of lime, without sensibly affecting the calcareous oxalate.

Distilled water from it.

Though the husk has a peculiar smell, it afforded nothing very remarkable by distillation in a water bath. I obtained only a liquor with a faintish taste, which, instead of coming over limpid, was brownish; and on its surface were perceptible slight iridescent pellicles, which sunk to the bottom in the form of a sediment.

Ashes.

The husk yielded by incineration potash, carbonate of lime, phosphate of lime, and oxide of iron.

From this examination it appears, that the fleshy covering of the walnut contains:

Substances contained in the husk.

1st, Starch:

2d, An acid and bitter substance, very alterable, which appears to approach the state of charcoal by the contact of air:

3d, malic acid:

4th, tannin:

5th, citric

5th, citric acid:
6th, phosphate of lime:
7th, oxalate of lime:
8th, potash.

XII.

Analyses of Minerals: by Martin Henry Klaproth, *Ph. D. &c.*

(*Continued from p.* 312.

Phosphorescent earth of Marmarosch.

PHOSPHORESCENT earth from Marmarosch.

Phosphoric acid	32·25
Fluoric acid	2·50
Lime	47
Silex	0·50
Oxide of iron	0·75
Water	1
Quartz mixed	11·50
	95·5

Combustible mineral from Prussia.

A new combustible mineral from East Prussia. 1000 grs. yielded by distillation.

	cubic inches.	Grs.
Carbonic acid gas	130 =	61·1
Carburetted hidrogen gas	320 =	59·5
Empyreumatic oil		90
Carbonate of ammonia		26·5
Water		385·5
The residuum consisting of		
Charcoal		228
Silex		45·5
Oxide of iron		14·5
Alumine		6
Phosphate of lime		14
Sulphate of lime		5
		935·6

A mi-

Mineral water of Riepoldsa.

A mineral water at Riepoldsa in Furstemberg. 128 oz. yielded

	grs.		grs.
Sulphate of soda, dry	93	or crystallized	221·5
Muriate of soda, dry	5		
Carbonate of soda, dry	2		5·5
——— lime	81		
——— magnesia	2		
Oxide of iron	2		
Silex	3		
Carbonic acid gas 332 cub. in.			
	188		

Tantalite.

Tantalite (tantale oxidé ferro-manganésifère of Haüy).

Earth of tantalium	88
Oxidulated iron	10
Oxide of manganese	2
	100

Cyanite.

Cyanite from Airolo, on St. Gothard.

Alumine	55·5
Silex	43
Oxided iron	0·5
Potash, *a trace*.	
	99

Vitreous feldspar.

Vitreous feldspar, called sanidin, from Drachenfels.

Silex	68
Alumine	15
Oxide of iron	0·5
Potash	14·5
Loss	2
	100

Agalmatolite.

Agalmatolite from Nagyag.

Silex	54·50
Alumine	34
Oxide of iron	0·75
Potash	6·25
Water	4
	99·5

Soaprock

Soaprock from Cornwall. Soapstone.

Silex	45
Magnesia	24·75
Alumine	9·25
Oxide of iron	1
Potash	0·75
Water	18
	98·75

Axinite. Axinite.

Silex	50·50
Lime	17
Alumine	16
Oxide of iron	9·50
——— manganese	5·25
Potash	0·25
	98·5

Gray semiopal from Moravia. Gray semiopal.

Silex	85
Alumine	3
Oxide of iron	1·75
Charcoal	1
Water, a little ammoniacal	8
Bituminous oil	0·33
	99·08

Bronzite*. Bronzite.

Silex	60
Magnesia	27·5
Oxide of iron	10·5
Water	0·5
	98·5

* See Journal, vol. xxv, p. 381.

Hyperstene

Labrador hornblende.

Hypersten (Labrador hornblende of Werner)*.

Silex	54·25
Magnesia	14
Alumine	2·25
Lime	1·50
Oxide of iron	24·50
Water	1
Oxide of manganese, *a trace*	
	97·5

Zoisite.

Zoisite.

Silex	44
Alumine	32
Lime	20
Oxide of iron	2·5
——— manganese, *a trace*	
	98·5

Natrolite.

Natrolite.

Silex	48
Alumine	24·25
Oxide of iron	1·75
Soda	16·50
Water	9
	99·5

Stangenstein.

Pycnite †.

Silex	43
Alumine	49·5
Oxide of iron	1
Fluoric acid	4
Water	1
Loss	1·5
	100

* See Journ. vol. xxvii, p. 153. † Ib. p. 154.

Lamellar

Lamellar talc from St. Gothard *. Lamellar talc.

Silex	62
Magnesia	30·50
Oxide of iron	2·50
Potash	2·75
Loss in roasting	0·50
	98·25

Common mica from Zinnwalde †. Common mica.

Silex	47
Alumine	20
Oxide of iron	15·50
——— manganese	1·75
Potash	14·50
	98·75

Muscovy glass, or mica in large laminæ from Siberia ‡. Mica in large laminæ.

Silex	48
Alumine	34·25
Oxide of iron	4·50
Magnesia	0·50
Potash	8·75
Loss in roasting	1·25
	97·25

Black mica from Siberia §. Black mica.

Silex	42·5
Alumine	11·5
Magnesia	9
Oxide of iron	22
——— manganese	2
Potash	10
Loss in roasting	1
	97

* See Journ. vol. xxvii, p. 226. † Ib. p. 227.
‡ Ib. p. 228. § Ib. p. 230.

Black

Black staurolite.

Black staurolite *.

Silex	37·50
Alumine	41
Oxide of iron	18·25
——— manganese	0·50
Magnesia	0·50
	97·75

Red staurolite.

Red staurolite from St. Gothard †.

Silex	27
Alumine	52·25
Oxide of iron	18·25
——— manganese	0·25
	97·75

Reddish tourmalin from Moravia.

Rubellite from Roschna, where it is found with lepidolite ‡.

Silex	43·50
Alumine	42·25
Oxide of manganese	1·50
Lime	0·10
Soda	9
Water	1·25
Loss	2·40
	100

Blue calcareous stone from Vesuvius.

Blue calcareous stone from Vesuvius.

Lime	58
Carbonic acid	28·50
Ammoniacal water	11
Magnesia	0·50
Oxide of iron	0·25
Charcoal	0·25
Silex	1·25
	99·75

(To be concluded in a future Number.)

* Journ. vol. xxvii, p. 152. † Ib. ‡ Ib. p. 154.

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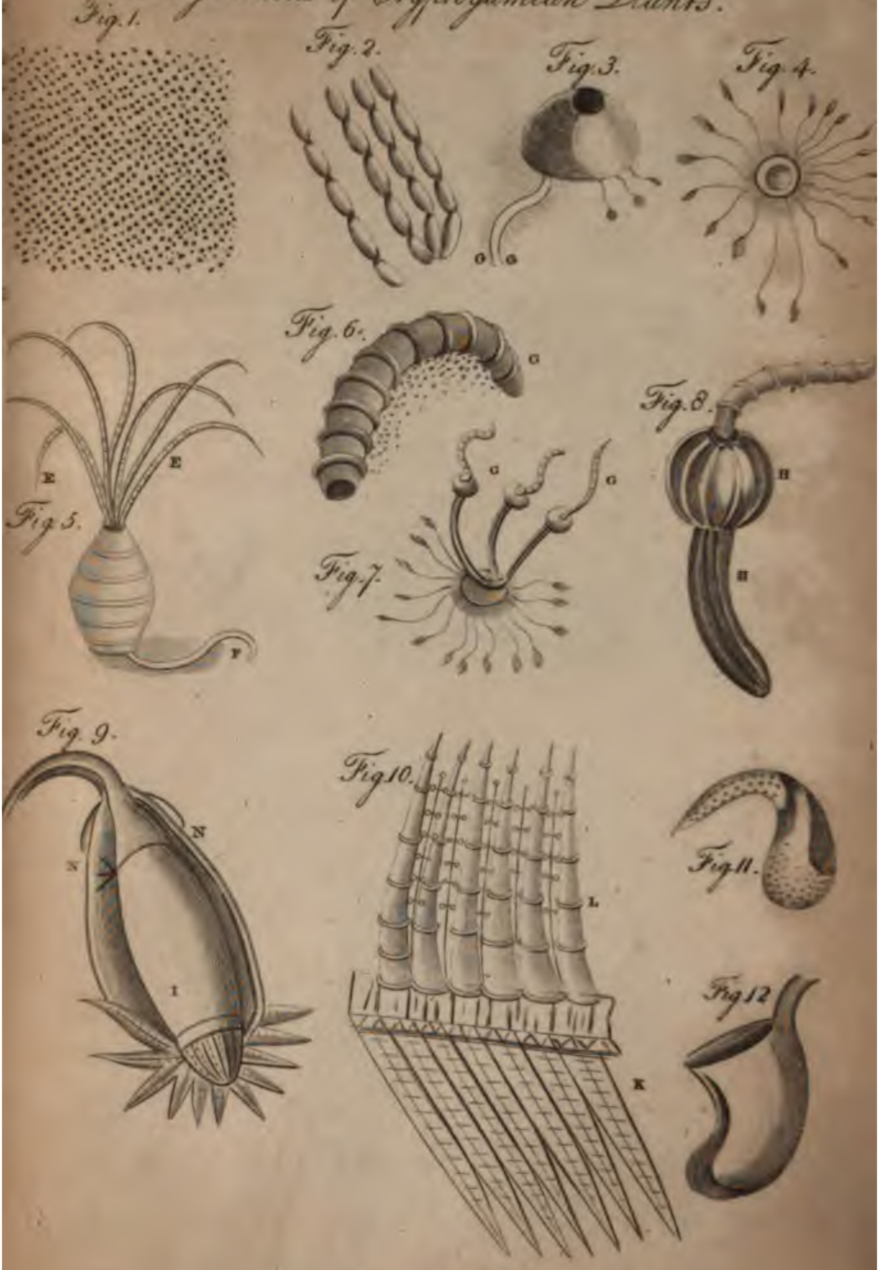
Stratford, Printer, Crown Court, Temple Bar.

Dissections of Cryptogamian Plants.



Dissections of Cryptogamian Plants.

Mr. E. Lydiatt's Smicrologometer.

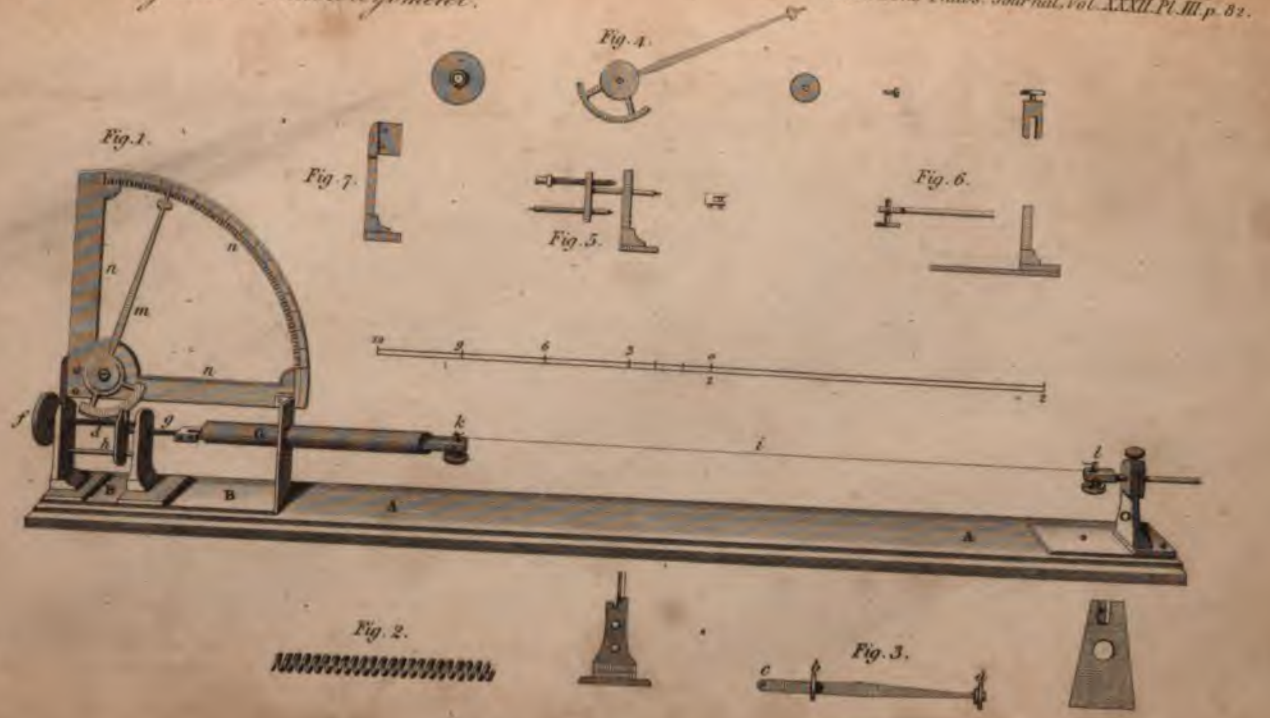


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Fig. 6.

Fig. 7.

Fig. 6.

Fig. 7.

Dissection of a branch of Laburnum by Mrs. A. Ibbetson

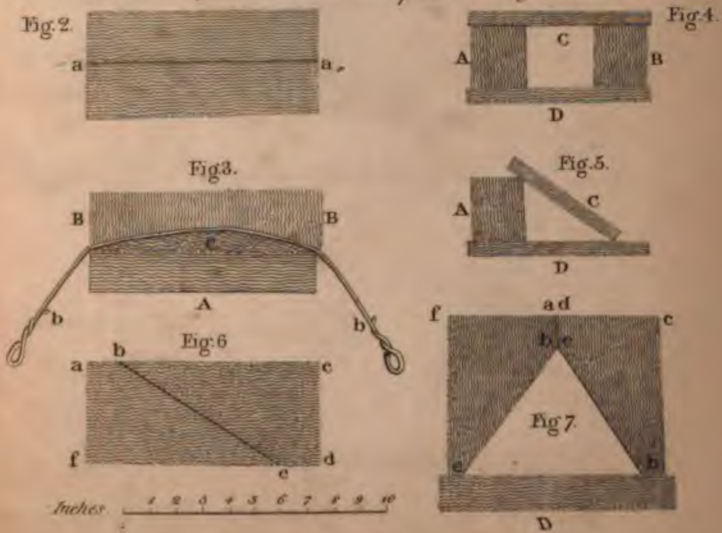
Fig. 1.

Fig. 2.

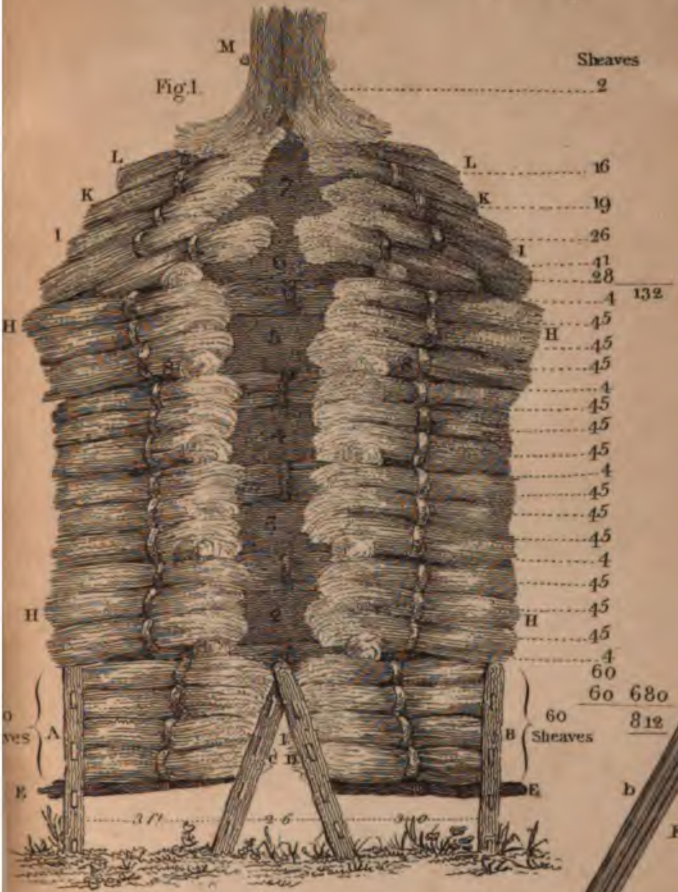
Fig. 3.

Foundation of Mr. Wm. Jones's Temporary Corn Rick.

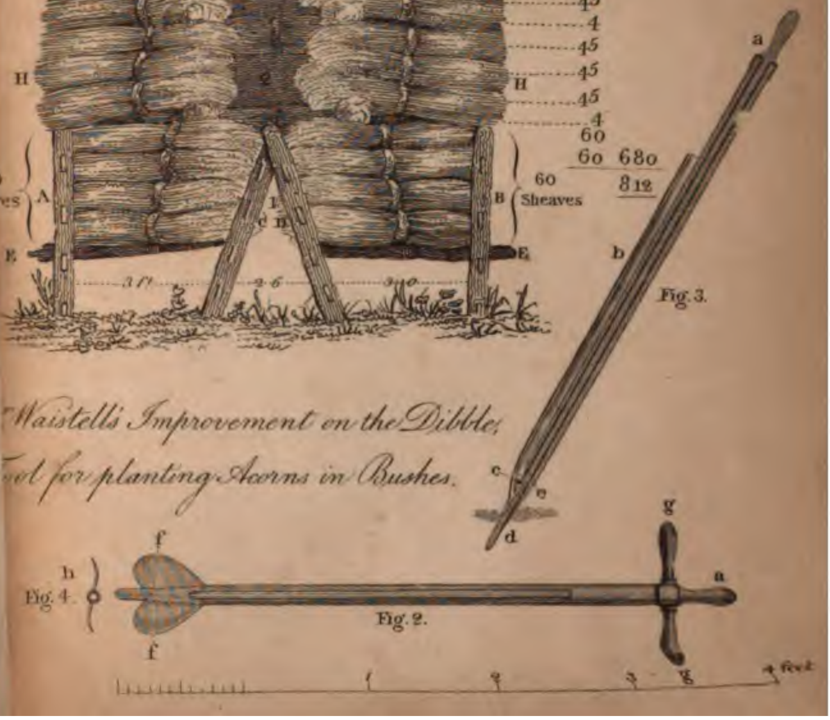
Mr. Stephens's Method of dividing Bricks



Section of Mr. Wm. Jones's temporary Corn Rick.



Mr. Waistell's Improvement on the Dibble;
Tool for planting Acorns in Bushes.



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Improvement in Fowling Pieces.

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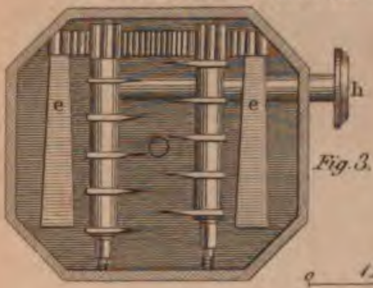

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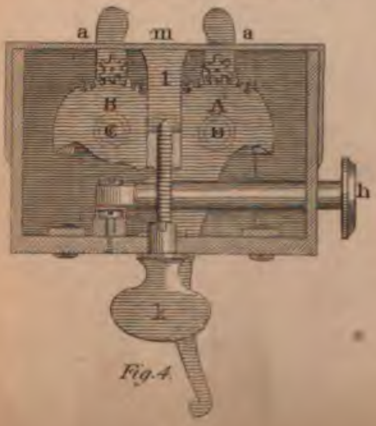

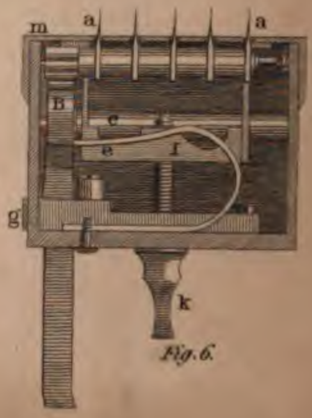

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